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CONCERNING THE FIGURE AND THE DIMENSIONS OF THE UNIVERSE OF SPACE[1]

THERE is something a little incongruous in attempting to consider the subject of this address in a theater or lecture hall whose roof and walls shut out from view the wide expanses of the world and the azure deeps. For how can we, amid the familiar finite scenes of a closed and blinded room, command a fitting mood for contemplating the infinite scenes without and beyond? A subject that has sheer vastness for its central or major theme demands for its appropriate contemplation the still expanse of some vast and open solitude, such as the peak of a lone and lofty mountain would afford, where the gaze meets no wall save the far horizon and no roof but the starry sky. Perhaps you will be good enough for the time to transport yourselves, in imagination, into the stillness of such a solitude, so that in the musing spirit of the place the questions to be propounded for consideration here may arise naturally and give us a due sense of their significance and impressiveness. What are the dimensions and what is the figure of our universe of space? How big is it and what is its shape? What is the figure of it and what is its size?

I do not mind owning that these questions have haunted me a good deal from the days of my youth. It happened in

[1] An address delivered under the auspices of the local chapters of the Society of Sigma Xi at the state universities of Minnesota, Nebraska and Iowa, April 24, 28 and 30, respectively, and at a joint meeting of the chapters of Sigma Xi and Phi Beta Kappa of Columbia University, May 8.

those days, though I was not aware of it nor became aware of it till after many years, that there were then coming into mathematics, just entering the fringe, so to speak, or the vestibule of the science, certain striking ideas which, as I venture to hope we may see, were destined, if not indeed to enable us to answer the questions with certainty, at all events to clarify them, to enrich their meaning and to make it possible to discuss them profitably. It has not been my fortune to meet many persons who had seriously propounded the questions to themselves or who seemed to be immediately interested in them when propounded by others—not many, even among astronomers, whose minds, it may be assumed, are specially "accustomed to contemplation of the vast." And so I have been forced to the somewhat embarrassing conclusion that my own long interest in the questions has been due to the fact of my being of a specially practical turn of mind. Quite seriously I venture to say that we are here engaged in a practical enterprise. For even if the questions were in the nature of the case unanswerable, which we do not admit, who does not know how great the boons that have come to men through pursuit of the unattainable? And who does not know that, as Mr. Chesterton has said, if you wish really to know a man, the most practical question to ask is, not about his occupation or his club membership or his party or church affiliations, but what are his views of the all-embracing world? What does he think of the universe? Do but fancy for a moment that in somewise men should come to *know* the exact shape or figure and especially the exact size or dimensions of the all-immersing space of our universe. It requires but little imagination, not much reflection, no extensive knowledge of cosmogonic history and speculation, no very profound insight into the ways of truth to men; it needs, I say, but little philosophic sense to see that such knowledge would in a thousand ways, direct and indirect, react powerfully upon our whole intelligence, upon all our attitudes, sentiments and views, transforming our theology, our ethics, our art, our religion, our philosophy, our literature, our science, and therewith affect profoundly the whole sense and manner, the tone, color and meaning, of all our institutions and the affairs of daily life. Nothing is quite so practical, in the sense of being effectual and influential, as the views men hold, consciously or unconsciously, regarding the great locus of their lives and their cosmic home.

In order to discuss the questions before us intelligibly and profitably it is not necessary by way of clearing the ground to enter far into metaphysical speculation or into psychological analysis with a view to ascertaining what it is that we mean or ought to mean by space. We are not obliged to dispute, much less decide, whether space is subjective or objective or both or indeed something that, as Plato in the "Timæus" acutely contends, is neither the one nor the other. We may or may not agree with the contention of Kant that space is, not an object, but the form, of outer sense; we may or may not agree with the radically different contention of Poincaré that (geometric as distinguished from sensible) space is nothing but what is known in mathematics as a group, of which the concept "is imposed on us, not as form of our sense, but as form of our understanding." It is, I say, not necessary for us, in the interest of soundness and intelligibility, to try to compose such differences or to attempt a settlement of these profound and important questions. As to the distinction between

sensible space and geometric space, it would indeed be indispensable to draw it sharply and to keep it always in mind, if we were undertaking to ascertain what the subject (or the object) of geometry is, or, what is tantamount, if we were seeking to get clearly aware of what it is that geometry is about. But in discussing the subject before us it is unnecessary to be always guarding that distinction; for, whilst it is the space of geometry, and not sensible space, that we shall be talking about, yet it would be a hindrance rather than a help if we did not allow, as we habitually do allow, the two varieties of space—the imagery of the one, the conceptual characters of the other—to mingle freely in our thinking. There will be finesse enough for the keenest arrows of our thought without our going out of the way to find it. A procedure less sophisticated will suffice. It will be sufficient to regard space as being what, to the layman and to the student of natural science, it has always seemed to be: a vast region or room round about us, an immense exteriority, locus of all suspended and floating objects of outer sense, the whence, where and whither of motion, theater, in a word, of the ageless drama of the physical universe. In naturally so construing the term we do not commit ourselves to the philosophy, so-called, of common sense; we thus merely save our discourse from the encumbrance of needless refinements; for it is obvious that, if space be not indeed what we have said it seems to be, the seeming is yet a fact, and our questions would remain without essential change: what, then, we should ask, are the dimensions and what is the figure of that seeming?

Though all the things contained within that triply extended spread or expanse which we call space are subject to the law of ceaseless change, the expanse itself, the container of all, appears to suffer no variation whatever, but to be, unlike time, a genuine constant, the same yesterday, today and forever, sole absolute invariant under the infinite host of transformations that constitute the cosmic flux. Whether it be so in fact, of course we do not know. We only know that no good reason has ever been advanced for holding the contrary as an hypothesis.

And yet there is a sense, which we ought I think to notice, an interesting sense, in which space seems to be, not a constant, but, like time, a variable. There is a sense, deeper and juster perhaps than at first we suspect, in which the space of our universe has in the course of time alternately shrunken and grown. During the last century, for example, it has, so it seems, greatly grown, in response, it may be, to an increasing need of the human mind. By grown I do not mean grown in the usual sense, I do not mean the biological sense, I do not mean the sense that was present to the mind of that great man, Leonardo da Vinci, when he wrote in effect as follows: if you wish to know that the earth has been growing, you have only to observe "how, among the high mountains, the walls of ancient and ruined cities are being covered over and concealed by the earth's increase"; and, if you would learn how *fast* the earth is growing, you have only to set a vase, filled with pure earth, upon a roof; to note how green herbs will immediately begin to shoot up; to note that these, when mature, will cast their seeds; to allow the process to continue through repetition; then, after the lapse of a decade, to measure the soil's increase; and, finally, to multiply, in order to have thus determined "how much the earth has grown in the course of a thousand years." In this matter, Leo-

nardo was doubtless wrong. At all events current scientific views are against him. The earth, we know, has grown, but the growth has been by accretion, by addition from without, and not, in biologic sense, by expansion from within (unless, indeed, we adopt the beautiful hypothesis of the poet and physicist, Theodor Fechner, for which so hard-headed a scientific man as Bernhardt Riemann had so much respect, the hypothesis, namely, that the plants, the earth and the stars have souls). The myriad-minded Florentine was, we of to-day think, in error, his error being one of those brilliant mistakes that but few men have been qualified to make. But in saying that space has grown we do not mean that it has grown in the biologic sense of Leonardo nor yet in the sense of addition from without. We mean that it has grown as a thing in mind may grow, as a thing in thought may grow; we mean that it has grown in men's conception of it. That space has, in this sense, been enlarged prodigiously in the course of recent time is evident to all. It has been often said that the first grand discovery of modern times is the immense extension of the universe *in space.*'' It would be juster to say that the first grand achievement of modern science has been the immense extension of space itself, the prodigious enlargement of it, in the imagination and especially in the thought of men. If we will but take the trouble to recall vividly the Mosaic cosmogony, in terms of which most of us have but recently ceased to frame our sublimest conceptions of the vast; if we remind ourselves of Plato's ''concentric crystal spheres, the adamantine axis turning in the lap of necessity, the bands that held the heaven together like a girth that clasps a ship, the shaft which led from earth to sky, and which was paced by the soul in a thousand years''; if we compare these conceptions with our own; if we think of ''the fields from which our stars fling us their light,'' fields that are really near and yet are so far that the swiftest of messengers, capable of circling the earth eight times in a second, requires for its journey hither thousands of years; if we do but make some such comparisons, we shall begin to realize dimly that, compared with modern space—the space of modern thought—elder space—the space of elder thought—is indeed ''but as a cabinet of brilliants, or rather a little jewelled cup found in the ocean or the wilderness.''

Suppose that in fact space were thus, like time, not a constant, but a variable; suppose it were a mental thing growing with the growth of mind; an increasing function of increasing thought; suppose it were a thing whose enlargement is essential as a psychic condition or concomitant or effect of the progress of science; would not our questions regarding its figure and its dimensions then lose their meaning? The answer is, no; as rational beings we should still be bound to ask: what are the dimensions and what is the figure of space to date? That is not all. If these questions were answered, we could propound the further questions: whether the space so characterized—the space of the present—is adequate to the present needs of science, and whether it is not destined to yet further expansion in response to the future needs of thought.

Men do not feel, however, that such spatial enlargements as I have indicated are genuine enlargements of space. In spite of whatever metaphysics or psychology may seem obliged to say to the contrary, men feel that what is *new* in such an enlargement is merely an increase of enlightenment regarding something old; they

feel that what is new is, not an added vastness, but a discovery of a vastness that always was and always will be. Let us trust this feeling and, regarding space as constant from everlasting to everlasting, let us take the questions in their natural intent and form: what are the dimensions and what is the figure of our universe of space?

If you propound these questions to a normal student of natural science, say to a normal astronomer, his response will be—what? If you appear to him to be quite sincere and if, besides, he be in an amiable mood, his response will, not improbably, be a significant shrug of the shoulders, designed to intimate that his time is too precious to be squandered in considering questions that, if not meaningless, are at all events unanswerable. I maintain, on the contrary, that this same student of natural science and, indeed, all other normally educated men and women, have, as a part of their intellectual stock in trade, perfectly definite answers to both of the questions. I do not mean that they are aware of possessing such wealth nor shall I undertake to say in advance whether their answers be correct. What I am asserting and what, with your assistance, I shall endeavor to demonstrate, is that perfectly precise, very intelligent and perfectly intelligible answers to both of the questions are logically involved in what every normally educated mind regards as the securest of its intellectual possessions. In order to show that such answers are to be found embedded in the content of the normally educated mind and in order to lay them bare, it will be necessary to have recourse to the process of explication. Explication, however, is nothing strange to an academic audience. It is true, indeed, that we no longer derive the verb, to educate, from *educere,* but it is yet a fact, as every one knows, that a large part of education is *eduction*—the leading forth into light what is hidden in the familiar content of our minds.

What are those answers? I shall present them in the familiar and brilliant words of one who in the span of a short life achieved a seven-fold immortality: immortality as a physicist, as a philosopher, as a mathematician, as a theologian, as a writer of prose, as an inventor and as a fanatic. From this brief but "immortal" characterization I have no doubt that you detect the author at once and at once recall the words: *Space is an infinite sphere whose center is everywhere and whose surface is nowhere.*

You will observe that, without change of meaning, I have substituted "space" for "universe" and "surface" for "circumference." This brilliant *mot* of Blaise Pascal, as every one knows, has long been valued throughout the world as a splendid literary gem. I am not aware that it has been at any time regarded seriously as a scientific thesis. It may, however, be so regarded. I propose to show, with your cooperation, that this exquisite saying of Pascal expresses with mathematical precision the firm, albeit unconscious, conviction of the normally educated mind respecting the size and the shape of the space of our universe. Be good enough to note carefully at the outset the cardinal phrases: *infinite sphere, center everywhere, surface nowhere.*

If you are told that there is an object completely enclosed and that the object is equally distant from all parts of the enclosing boundary or wall, you instantly and rightly think of a sphere having that object as center. Let me ask you to think of some point, any convenient point, P, together with all the straight lines or rays—called a sheaf of lines or rays—that, beginning at P, run out from it as far as ever the nature of space allows. We ask: do all the rays of the sheaf run out equally far? It seems

perfectly evident that they do, and with this we might be content. It will be worth while, however, to examine the matter a little more attentively. Denote by L any chosen line or ray of the sheaf. Choose any convenient unit of length, say a mile. We now ask: how many of our units, how many miles can we, starting from P, lay off along L? Lay off, I mean, not in fact, but in thought. In other words: how many steps, each a mile long, can we, in traversing L, take in thought? Hereafter let the phrase "in thought" be understood. Can the question be answered? It can. Can it be answered definitely? Absolutely so. How? As follows. Before proceeding, however, let me beg of you not to hesitate or shy if certain familiar ideas seem to get submitted to the logical process—the mind-expanding process—of generalization. There is to be no resort to any kind of legerdemain. Let us be willing to transcend imagination, and, without faltering, to follow thought, for thought, free as the spirit of creation, owns no bar save that of inconsistence or self-contradiction. Consider the sequence of cardinal numbers,

$$(S)\quad 1, 2, 3, 4, 5, 6, 7, \ldots.$$

The sequence is neither so dry nor so harmless as it seems. It has a beginning; but it has no end, for, by the law of its formation, after each term there is a next. The difference between a sequence that stops somewhere and one that has no end is awful. No one, unless spiritually unborn or dead, can contemplate that gulf without emotions that take hold of the infinite and everlasting. Let us compare the sequence with the ray L of our sheaf. Choose in (S) any number n, however large. Can we go from P along L that number n of miles? We are certain that we can. Suppose the trip made, a mile post set up and on it painted the number n to tell how far the post is from P. As n is any number in (S), we may as well suppose, indeed we have already implicitly supposed, mile posts, duly distributed and marked, to have been set up along L to match each and every number in the sequence. Have we thus set up all the mile posts that L allows? We are certain that we have, for, if we go out from P along L any possible but definite number of miles, we are perfectly certain that that number is a number in the sequence, and that accordingly the journey did but take us to a post set up before. What is the upshot? It is that L admits of precisely as many mile posts as there are cardinal numbers, neither more nor less. How long is L? The answer is: L is exactly as many miles long as there are integers or terms in the sequence (S). Can we say of any other line or ray L' of the sheaf what we have said of L? We are certain that we can. Indeed we have said it, for L was *any* line of the sheaf. May we, then, say that any two lines, L and L', of the sheaf are *equal*? We may and we must. For, just as we have established a one-to-one correspondence between the mile posts of L and the terms of (S), so we may establish a one-to-one correspondence between the mile posts of L and those of L', and what we mean by the *equality* of two classes of things is precisely the possibility of thus setting up a one-to-one correlation between them. Accordingly, all the lines or rays of our sheaf are equal. We can not fail to note that thus there is forming in our minds the conception of a sphere, centered at P, larger, however, than any sphere of slate or wood or marble—a sphere, if it be a sphere, whose radii are the rays of our sheaf. Is not the thing, however, too vast to be a sphere? Obviously yes, if the lines or rays of the sheaf have a length that is indefinite, unassignable; obviously no, if their length be assignable and definite. We have

seen the length of a ray contains exactly as many miles as there are integers or terms in (S). The question, then, is: has the totality of these terms a definite assignable number? The answer is, yes. To show it, look sharply at the following fact, a bit difficult to see only because it is so obvious, being writ, so to speak, on the very surface of the eye. I wish, in a word, to make clear what is meant by the cardinal number of any given class of things. The fingers of my right hand constitute a class of objects; the fingers of my left hand, another class. We can set up a one-to-one correspondence between the classes, pairing the objects in the one with those in the other. Any two classes admitting of such a correlation are said to be *equivalent*. Now given any class K, there is another class C composed of all the classes each of which is equivalent to K. C is called the cardinal number of K, and the name of C, if it have received a name, tells how many objects are in K. Thus, if K is the class of the fingers of my right hand, the word *five* is the name of the class of classes each equivalent to K. Now to the application. The terms of (S) constitute a class K (of terms). Has it a definite number? Yes. What is it? It is the class of all classes each equivalent to K. Has this number-class received a name of its own? Yes, and it has, like many other numbers, received a symbol, namely, $\aleph_0$, read Aleph null. It is, then, this cardinal number Aleph, not familiar, indeed, but perfectly definite as denoting a definite class, it is this that tells us how many terms are in (S) and therewith tells us the length of the rays of our sheaf. Herewith the concept that was forming is now completely formed: *space is a sphere centered at P.*

But is the sphere, as Pascal asserts, an *infinite* sphere? We may easily see that it is. Again consider the sequence (S) and with it the similar sequence (S'),

$$(S)\quad 1,\ 2,\ 3,\ 4,\ 5,\ 6,\ 7,\ \ldots,$$
$$(S')\quad 2,\ 4,\ 6,\ 8,\ 10,\ 12,\ 14,\ \ldots.$$

Observe that all the terms in (S') are in (S) and that (S) contains terms that are not in (S'). (S') is, then, a proper *part* of (S). Next observe that we can pair each term in (S) with the term below it in (S'). That is to say: the whole, (S), is equivalent to one of its parts, (S'). A class that thus has a part to which it is equivalent is said to be infinite, and the number of things in such a class is called an infinite number. Aleph is, then, an infinite number, and so we see that the rays of our sheaf, the radii of our sphere, are infinite in length: *space is an infinite sphere entered at P.*

Finally, what of the phrases, *center everywhere, surface nowhere?* Can we give them a meaning consistent with common usage and common sense? We can, as follows. Let O be any chosen point somewhere in your neighborhood. By saying that the center P is everywhere we mean that P may be taken to be *any* point within a sphere centered at O and having a finite radius, a radius, that is, whose length in miles is expressed by any integer *in* (S). And by saying that the surface of our infinite sphere is nowhere we mean that no point of the surface can be reached by traveling out from P any *finite* number, however large, of miles, by traveling, that is, a number of miles expressed by any number, however large, in (S).

Here we have touched our primary goal: we have demonstrated that men and women whose education, in respect of space, has been of normal type, believe profoundly, albeit unawares, that the space of our universe is an infinite sphere of which the center is everywhere and the surface nowhere. Such is the beautiful conception,

the great conception—mathematically precise yet mystical withal and awful in its limitless reaches—which is ever ready to form itself, in the normally educated mind and there to stand a deep-rooted conscious conviction regarding the shape and the size of the all-embracing world.

Is the conception valid? Does the conviction correspond to fact? Is it true? It is not enough that it be intelligible, which it is; it is not enough that it be noble and sublime, which also it is. No doubt whatever is noble and sublime is, in some sense, true. For we mortals have to do with more than reason. Yet science, science in the modern technical sense of the term, having elected for its field the domain of the rational, allows no superrational tests of truth to be sufficient or final. We must, therefore, ask: are the dimensions and the figure of our space, in fact, what, as we have seen, Pascal asserts and the normally educated mind believes them to be? Long before the days of Pascal, back yonder in the last century before the beginning of the Christian era, one of the acutest and boldest thinkers of all time, immortal expounder of Epicurean thought, answered the question with the utmost confidence in the affirmative. I refer to Lucretius and his "De Rerum Natura." In my view that poem is the greatest and finest union of literary excellence and scientific spirit to be found in the annals of human thinking. I maintain that opinion of the work despite the fact that the majority of its conclusions have been invalidated by time, have perished by supersession; for we must not forget that, in respect of knowledge, "the present is no more exempt from the sneer of the future than the past has been." I maintain that opinion of the work despite the fact that the enterprise of Lucretius was marvelously extravagant; for we must not forget that the relative modesty of modern men of science is not inborn, but is only an imperfectly acquired lesson. Well, it is in that great work that Lucretius endeavors to prove that our universe of space is infinite in the sense that we have explained. His argument, which runs to many words, may be briefly paraphrased as follows. Conceive that, starting from any point of space, you go out in any direction as far as you please, and that then you hurl your javelin. Either it will go on, in which case there is space ahead for it to move in, or it will not go on, in which case there must be space ahead to contain whatever prevents its going. In either case, then, however far you may have gone, there is yet space beyond. And so, he concludes, space is infinite, and he triumphantly adds:

> Therefore the nature of room and the space of the unfathomable void are such as bright thunderbolts can not race through in their course though gliding on through endless tract of time, no nor lessen one jot the journey that remains to go by all their travel—so huge a room is spread out on all sides for things without any bounds in all directions round.

Such is the argument, the great argument, of the Roman poet. Great I call it, for it is great enough to have fooled all philosophers and men of science for two thousand years. Indeed only a decade ago I heard the argument confidently employed by an American thinker of more than national reputation. But is the argument really fallacious? It is. The conclusion may indeed be quite correct—space may indeed be infinite, as Lucretius asserts—but it does not follow from his argument. To show the fallacy is no difficult feat. Consider a sphere of finite radius. We may suppose it to be very small or intermediate or very large—no matter what its size so long as its radius is finite. By sphere, in this part of the discussion, I shall mean sphere-surface. Be good enough

to note and bear that in mind. Observe that this sphere—this surface—is a kind of room. It is a kind of space, region or room where certain things, as points, circle arcs and countless other configurations can be and move. These things, confined to this surface, which is their world, their universe of space, if you please, enjoy a certain amount, an immense amount, of freedom: the points of this world can move in it hither, thither and yonder; they can move very far, millions and millions of miles, even in the same direction, if only the sphere be taken large enough. I see no reason why we should not, for the sake of vividness, fancy that spherical world inhabited by two-dimensional intelligences conformed to their locus and home just as we are conformed to our own space of three dimensions. I see no reason why we should not fancy those creatures, in the course of their history, to have had their own Democritus and Epicurus, to have had their own Roman republic or empire and in it to have produced the brilliant analogues of our own Vergil, Cicero and Lucretius. Do but note attentively—for this is the point—that their Lucretius could have said about their space precisely what our own said about ours. Their Lucretius could have said to his fellow-inhabitants of the sphere: "starting at any point, go as far as ever you please in any straight line"—such line would of course (as *we* know) be a great circle of the sphere—"and then hurl your javelin"—the javelin would, as *we* know, be only an *arc* of a great circle—"either it will go on, in which case, etc.; or it will not, etc."; thus giving an argument identical with that of our own Lucretius. But what could it avail? *We* know what would happen to the javelin when hurled as supposed in the surface: it would go on for a while, there being nothing to prevent it. But whether it went on or not, it could not be logically inferred that the surface, the space in question, is infinite, for we know that the surface is finite, just so many, a finite number of, square miles. The fallacy, at length, is bare. It consists—the fact has been recently often pointed out—in the age-long failure to distinguish adequately between *unbegrenzt* and *unendlich* —between *boundless* and *infinite* as applied to space. What our fancied Lucretius proved is, if anything, that the sphere is boundless, but not that it is infinite. What our real Lucretius proved is, if anything, that the space of our universe is boundless, but not that it is infinite. That a region or room may be boundless without being infinite is clearly shown by the sphere (surface). How evident, once it is drawn, the distinction is. And yet it was never drawn, in thinking about the dimensions of space, until in 1854 it was drawn by Riemann in his epoch-marking and epoch-making *Habilitationschrift* on the foundations of geometry.

What, then, is the fact? Is space finite, as Riemann held it may be? Or is it infinite, as Lucretius and Pascal deliberately asserted, and as the normally educated mind, however unconsciously, yet firmly believes? No one knows. The question is one of the few great outstanding scientific questions that intelligent laymen may, with a little expert assistance, contrive to grasp. Shall we ever find the answer? Time is long, and neither science nor philosophy feels constrained to haul down the flag and confess an *ignorabimus*. Neither is it necessary or wise for science and philosophy to camp indefinitely before a problem that they are evidently not yet equipped to solve. They may proceed to related problems, always reserving the right to return with better instruments and added light.

In the present instance, let us suppose,

for the moment, that Lucretius, Pascal and the normally educated mind are right: let us suppose that space is infinite, as they assert and believe. In that case the bounds of the universe are indeed remote, and yet we may ask: are there not ways to pass in thought the walls of even so vast a world? There are such ways. But where and how? For are we not supposing that the walls to be passed are distant by an amount that is infinite? And how may a boundary that is infinitely removed be reached and overpassed? The answer is that there are many infinites of many orders; that infinites are surpassed by other infinites; that infinites, like the stars, differ in glory. This is not rhetoric, it is naked fact. One of the grand achievements of mathematics in the nineteenth century is to have defined infinitude (as above defined) and to have discovered that infinites rise above infinites, in a genuine hierarchy without a summit. In order to show how we can in thought pass the Lucretian and Pascal walls of our universe, I must ask you to assume as a lemma a mathematical proposition which has indeed been rigorously established and is familiar, but the proof of which we can not tarry here to reproduce. Consider all the real numbers from *zero* to *one* inclusive, or, what is tantamount, consider all the points in a unit segment of a continuous straight line. The familiar proposition that I am asking you to assume is that it is not possible to set up a one-to-one correspondence between the points of that segment and the positive integers (in the sequence above given), but that, if you take away from the segment an infinitude (Aleph) of points matching all the positive integers, there will remain in the segment more points, infinitely more, than you have taken away. That means that the infinitude of points in the segment infinitely surpasses the infinitude of positive integers; surpasses, that is, the infinitude of mile posts on the radius of our infinite (Pascal) sphere. Now conceive a straight line containing as many miles as there are points in the segment. You see at once that in that conception you have overleaped the infinitely distant walls of the Lucretian universe. Overleaped, did I say? Nay, you have passed beyond those borders by a distance infinitely greater than the length of any line contained within them. And thus it appears that, not our imagination, indeed, but our reason may gaze into spatial abysses beside which the infinite space of Lucretius and Pascal is but a meager thing, infinitesimally small. There remain yet other ways by which we are able to escape the infinite confines of this latter space. One of these ways is provided in the conception of hyperspaces enclosing our own as this encloses a plane. But that is another story, and the hour is spent.

The course we have here pursued has not, indeed, enabled us to answer with final assurance the two questions with which we set out. I hope we have seen along the way something of the possibilities involved. I hope we have gained some insight into the meaning of the questions and have seen that it is possible to discuss them profitably. And especially I hope that we have seen afresh, what we have always to be learning again, that it is not in the world of sense, however precious it is and ineffably wonderful and beautiful, nor yet in the still finer and ampler world of imagination, but it is in the world of conception and thought that the human intellect attains its appropriate freedom—a freedom without any limitation save the necessity of being consistent. Consistency, however, is only a prosaic name for a limitation which, in

another and higher realm, harmony imposes even upon the muses.

CASSIUS J. KEYSER
COLUMBIA UNIVERSITY

CLINICAL PSYCHOLOGY: WHAT IT IS AND WHAT IT IS NOT

ON an occasion like this[1] it would seem proper, representing as I do one of the newest of the sciences, that I address myself to some of the basic questions of this science. Perhaps the very first question with which one is confronted is simply this: "In view of the rapid multiplication of the sciences, by what right does clinical psychology lay claim to an independent existence?" That is a question which may perturb some sensitive minds, but it does not disconcert the clinical psychologist, for he regards the question as perfectly legitimate and capable of satisfactory answer.

It is just and proper that a new claimant to membership in the family of sciences should be required to present her credentials. It is a natural human trait to challenge or contest the claims of a newcomer. It has ever been thus. Every branch of knowledge before winning recognition as an independent science has been forced to demonstrate that it possesses *a distinct and unique body of facts* not adequately treated by any other existing science; or that it approaches the study of a *common body* of facts from a *unique* point of view, and with methods of its own. Psychology, biochemistry, dentistry, eugenics, historiometry and many other sciences have been thus obliged to fight their way inch by inch to recognition as independent sciences. It is not long since physiology claimed psychology as its own child and stoutly contested her rights to existence; nor is it long since medicine denied any right to independent existence to dentistry. It is no surprise that a number of sciences now claim clinical psychology as part and parcel of their own flesh and blood, and that they deny her the right to "split off from the parent cell" and establish an unnursed existence of her own. Just as nature abhors a vacuum, so science abhors the multiplication of sciences; just as the big corporation octopus in the industrial world tries to get monopolistic control of the sources of production and distribution, so the various sciences, naturally insatiable in their desire for conquest, attempt only too often to get monopolistic control of all those elements of knowledge which they may be able to use for their own aggrandizement, whether or not they have developed adequate instruments for scientifically handling those elements.

Clinical psychology, however, is quite ready to contest the attempts to deprive her of her inalienable rights to the "pursuit of life and happiness." Fundamentally, she bases her claims to recognition as an independent science on the fact that

[1] Substance of an address delivered before the Conference on the Exceptional Child, held under the auspices of the University of Pittsburgh, April 16, 1912. Lest misapprehensions arise, it should be clearly understood that in this discussion I am concerned only with the relation of *clinical psychology* to *mentally* exceptional school children; and that I distinctly recognize a different type of exceptional children, namely, the *physical* defectives. The physical defectives should be examined by skilled pediatricians. The clinical psychologist is interested in physically exceptional children if they manifest mental deviations. Moreover, while I hold that the psycho-clinical laboratories must become the clearing houses for all types of *mentally* or *educationally* exceptional children in the schools, nearly all mentally exceptional children should be given a physical examination by consulting or associated medical experts. Physical abnormalities should, of course, be rectified, whether or not it can be shown that they sustain any causal relation to any mental deviations which may have been disclosed in the psycho-clinical examination. They may claim treatment in their own right.

she *does* possess a unique body of facts not adequately handled by any existing science, and that she investigates these facts by methods of her own. These facts consist of *individual mental variations,* or the phenomena of *deviating or exceptional mentality.* In other words, clinical psychology is concerned with the *concrete* study and examination of the behavior of the *mentally exceptional individual* (not groups), by its own methods of observation, testing and experiment.

In the study or examination of individual cases, the clinical psychologist seeks to realize four fundamental aims:

1. *An Adequate Diagnosis or Classification.*—He attempts to give a correct description of the nature of the mental deviations shown by his cases; he tries to determine whether they are specific or general, whether they affect native or acquired traits; he attempts to measure by standard objective tests the degree of deviation of various mental traits or of the general level of functioning; he seeks to arrive at a comprehensive clinical picture, to disentangle symptom-complexes and to reduce the disorders to various reaction types.

2. *An Analysis of the Etiological Background.*—His examination is bent not only on determining the present mental status of the case, but on discovering the causative factors or agents which have produced the deviations—whether these factors are physical, mental, social, moral, educational, environmental or hereditary. In order to arrive at a correct etiology the psycho-clinician makes not only a cross-section analysis of the case, but also a longitudinal study of the evolution of the deviation or symptom-complex. Therefore he does not limit himself merely to a psychological examination, but requires a dento-medical examination and a sociological and hereditary examination. The physical examination should be made by experts in dentistry and in the various specialties in the field of medicine. The psycho-clinicist, however, should be so trained in physical diagnosis that he can detect the chief physical disorders, so that he can properly refer his cases for expert physical examination.

3. A determination of the *modification* which the disorder has wrought in the *behavior of the individual.* He should determine what its consequences have been: what effects it has had upon his opinions, beliefs, thoughts, disposition, attitudes, interests, habits, conduct, capacity for adaptation, learning ability, capacity to acquire certain kinds of knowledge or various accomplishments, or to do certain kinds of school work. He should seek to locate the conflicts between instincts and habits which may have been caused by the deviations.

4. The determination of the *degree of modifiability* of the variations discovered. Can the deviations be corrected or modified, and if so to what extent and by what kinds of orthogenic measures? A clinical psychologist is no less a scientific investigator than a consulting specialist; he diagnoses in order to prognose and prescribe. His aim first and last is eminently practical.

Basis of Selection of Cases

The clinical psychologist selects his cases not so much on the nature of the cause of the deviations as on the nature of the deviations themselves, and the nature of the treatment. He is interested in cases which, first of all, depart from the limits of mental normality. ***Exceptional mentality,*** or, if you please, ***mental exceptionality,*** is his first criterion. In the second place, he is interested in those cases in which the nature of the treatment—the *process* of *righting* the mental variations, of straightening out the deviations, the orthogenesis—

is wholly or chiefly or partly *educational.* In the term educational I include training of a hygienic, physiological (in Seguin's sense), pedagogical, psychological, sociological or moral character.

GROUPING OF CASES

It is thus evident that the clinical psychologist may group his cases into two main classes.

A. Those in which the *mental variations are fundamental* or primary, and the physical disabilities only accessory or sequential. With these cases the treatment must be primarily educational and only secondarily medical. What types of children are included in this group?

I. *Feeble-minded Children.*—Feeble-mindedness formerly was regarded as an active disorder—a disease—and was accordingly treated exclusively medically. The theory of causation was wrong and so the results were unsatisfactory. Since the year 1800 (Itard, the apostle to the feeble-minded) and particularly since the year 1837 (Seguin, the liberator of the feeble-minded), it has become increasingly apparent that feeble-mindedness is an arrest of development; and accordingly since that time the condition has been primarily educationally treated instead of medically. This change in point of view has revolutionized the treatment of the feeble-minded. The person who did most to ameliorate their condition is Seguin, whose method, almost entirely educational, has served as the model for the effective institutional work for the feeble-minded done since his day, although we have outgrown various details of his system. Moreover, it served as the chief inspiring force for the constructive orthogenic work done for the feeble-minded within the last decade or so by Montessori. She, herself a physician, but with special training in psychology and pedagogy, tells us that in 1898 as a result of a careful study of the problem of feeble-mindedness she became persuaded that the problem was primarily a pedagogical and not a medical one. It is granted without question, of course, that there is a medical side to the care of the feeble-minded just as there is a medical side to the care of the normal child. Nay, owing to the heightened degree of susceptibility to disease and accidents found among the feeble-minded, the medical side looms larger in the care of the feeble-minded than in the care of normals. Indeed, no institution for the feeble-minded can be properly organized without an adequate staff of medical experts; but fundamentally the problem of the amelioration of the lot of feeble-minded children is an educational one—their hygienic, pedagogical and moral improvement, as well as their elimination by the method of colonization or sterilization.

II. *Retardates,* technically so-called—of which there are probably on a conservative estimate 6,000,000 in the schools of the United States. Some of these are retarded (1) merely pedagogically in a relative sense—relative to an *arbitrary curricular standard.* Many children do not fit the standard, because the standard itself is off the norm. It is largely a case of a misfit curriculum instead of a misfit child. So far as this class of misfits is concerned the problem is simply one of correct adjustment of the pedagogical demands of the curriculum.

A considerable percentage of the retardates, however, are retarded because of (2) *genuine mental arrest of development.* They are as truly arrested or deficient as the feeble-minded, but to a *lesser extent.* The difference is a *quantitative* and not a qualitative one, and the problem of correc-

tion consists fundamentally in providing a right educational regimen.

Then there is (3) a smaller proportion of retardates who are mentally retarded because of *environmental handicaps,* such as bad housing, home and neighborhood conditions, bad sanitation, lack of humidity, lack of pure air or excessive temperature in the schoolroom, vicious or illiterate surroundings, frequent moving or transfer, emigration which may cause linguistic maladaptation, etc. With such retardates the problem is partly sociological, partly hygienic, and partly pedagogical.

We have a final group of children (4) who are mentally retarded because of some *physical defect.* With children of this type the problem is partly medical and partly educational. The first efforts made in behalf of such children should be medical and hygienic. Undoubtedly the removal of physical handicaps will restore some pupils to normal mentality, while in the case of other pupils the results will be negative. Unfortunately there are very few scientific studies available of the orthophrenic effects of the correction of physical defects.[2] Many of the studies in this field have a questionable value because of the obvious, but evidently unconscious, bias of the investigators. Some desire to show favorable results and, therefore, unconsciously select only the favorable cases; others are swayed by the opposite motive and accordingly tend to select the negative cases. Hence, at the present time we find considerable diversity of opinion as to the orthogenic influences of the correction of physical disorders. The opinion of John J. Cronin, M.D., probably approximates the truth:

> The successes simply mean that a large number of children were perfect except for some one abnormality. . . . The alleviation of any single kind of physical handicap is merely one step towards the successful result sought, and many other factors must obtain before some measure of success is assured.

Likewise A. Emil Schmitt, M.D.:

> It should constantly be borne in mind that if every physical defect has been successfully removed the mental unbalance or deficiency can remain unaltered, inasmuch as it was primarily a mental defect and can be reached only by methods of education or psychological treatment.

While I am quite convinced that all mentally retarded children should undergo a careful physical examination, and that such physical corrective measures should be applied as are indicated by expert medical opinion, yet it needs to be re-emphasized that the removal of a physical disability is frequently only the first step toward restoration. If the child has fallen behind pedagogically or mentally, he will in many cases need special pedagogical attention if he is to catch step with the class procession; moreover, after a certain critical age has been passed the removal of physical obstructions exercises only a slight orthophrenic influence, and the reestablishment of effective mental functioning, if it can be done at all, will require the prolonged application of a special corrective pedagogy.

III. *The Super-normals.*—Both of the above types of children come on the minus side of the curve of efficiency. On the other side we find the plus deviates—the bright, brilliant, quick, gifted, talented, precocious children. These children may present no peculiarities on the physical side, if we except the type of nervously unstable, precocious children. With the supernormal

[2] However, see an attempt at the scientific measurement of the orthophrenic effects of the correction of dental defects in J. E. Wallace Wallin's "Experimental Oral Euthenics," *The Dental Cosmos,* 1912, pp. 404–413, 545–566. Also, "Experimental Oral Orthogenics," *The Journal of Philosophy, Psychology and Scientific Methods,* 1912, pp. 290–298.

child the problem is almost entirely an educational one: the introduction of schemes of flexible grading; of fast, slow and normal sections, and of supernormal classes; providing special opportunities for doing specialized work, and a special pedagogy, which should probably be as largely negative as positive. If there is any one child in our scheme of public education which has been neglected more than any other, it is the child of unusual talents. A nation can do no higher duty by its subjects than to provide those conditions which will rescue its incipient geniuses from the dead-level of enforced mediocrity.

IV. *Speech defectives,* particularly the two and one half per cent. (approximately) of stutterers and lispers who encumber our classes. In few fields of scientific research is it possible to find such astonishing diversity of so-called expert opinion as on the question of the causation of stuttering (or stammering). It is claimed to be a gastric, pneumogastric, lung, throat, lip, brain, hypoplastic, nervous and mental disorder. It is said to be a form of epilepsy, a form of hysteria, and a form of mental strife, or repression, between latent and manifest mental contents. Moreover, few writers show such a consummate genius for self-contradiction as writers on stuttering. Before me lies a reprint of a recent dissertation on the "Educational Treatment of Stuttering Children." The writer begins by saying that stuttering is a "pathological condition," a disease, and that therefore its treatment belongs to a specialist on diseases. The disease appears, however, on the second page to be merely "a purely functional neurosis," while on the last page the trouble is nothing more than a "mental one," caused by influences acting on the mind. As a matter of fact, the treatment which the writer recommends is through and through educational and largely psychological. It consists of certain physical exercises, designed not so much to strengthen certain organs as to win the patient's interest and restore his self-confidence; and certain psychotherapeutic and hypnotic exercises.

Waiving for the time being the nature of the cause, we can agree on one thing; namely, that the methods of treating stuttering (and lisping) which have been proved effective are almost exclusively educational. Many of the neurotic symptoms found in the stutterer are the results of mental tension and will disappear with the correction of the stuttering.

V. *Incipient psychotics,* or children who show developmental symptoms of mental disorders or mental alienation. Here we meet with the same controversy between the advocates, on the one hand, of a *somatogenic* theory, and, on the other hand, of a *psychogenic* theory of causation. While it must be admitted that many of the psychoses are certainly organic, others almost as certainly are functional and are produced by idiogenic factors (a view entertained by such well-known psychiatrists as Meyer, Freud, Janet, Dubois, Jones, Prince). Now, irrespective of whether the cause is chiefly physical or mental, it is being recognized by a number of the leading present-day psychiatrists that drug treatment for the majority of the insane, whether juvenile or adult, is secondary to the educational treatment. Instead of merely prescribing physical hygiene for the insane, leading alienists are now prescribing mental hygiene. The cure is being conceived in terms of a process of reeducation. Moreover, so far as concerns the mentally unstable child in the schools, the chief reliance is obviously on hygienic and educational guidance.

B. Cases in which the *physical devia-*

tions are fundamental or primary, and the mental variations sequential, but the remedy partly or chiefly educational. Here we include malnutrition, rickets, marasmus, hypothyroidism, tuberculosis, heart trouble, chorea and similar diseases. In all of these the treatment must be primarily medical, although there should be a special temporary educational regimen for these children. This group also includes the blind and the deaf. But here the treatment is almost wholly educational. The physical defects are incurable, but the mental defects can be partly overcome by proper compensatory educational treatment. The epileptic also must be added to this group. Epilepsy is evidently an active disorder or disease process, although the pathology is wrapped in the deepest obscurity. The epileptics appear like purely medical cases. The medical aspect certainly is important, but the records show that only from 5 to 10 per cent. are curable, and that the attacks can be as readily modified or regulated in most cases by proper hygienic treatment as by drug medication or surgical interference. Even with these unfortunates it can be said that the best results come from a proper medico-educational régimé—colonization, out-door employment, industrial schooling, bathing, etc.

SUMMARY OF IMPORTANT CONCLUSIONS

We are thus brought to the two following conclusions:

1. There is a set of unique facts—facts of individual mental variation—which no existing science has adequately treated. It is with these facts that the work of the clinical psychologist is concerned. Just as psychology became an independent science by demonstrating that it possessed a legitimate claim to a unique world of facts, so clinical psychology is ready to make her declaration of independence and dedicate herself to the investigation of a body of facts—facts of individual mental variation—not hitherto adequately handled by any existing science. It is concerned with the study of individual cases of deviate mentality, particularly with those types which are amenable to improvement or correction by psycho-educational processes.

2. The proper handling of these cases, whether for purposes of examination, recommendation or prescription, can only be done by a *psycho-educational specialist* who possesses a technical knowledge of educational and child psychology, of child hygiene, of the science and art of education, and of various classes of mental defectives or deviates. He should possess a thorough grounding in clinical procedure, particularly in the methods of clinical psychology, while he must also have a certain amount of training in pediatrics, physical diagnosis, neurology and psychiatry. He must be thoroughly skilled in the differential pedagogy appertaining to various types of mentally exceptional children.

C. The Relations of Clinical Psychology —Some Affirmations and Denials.—There is a number of sciences with which clinical psychology is, will be, or should be, closely related, but which are not synonymous with clinical psychology.

1. Clinical psychology is not the same as *psychopathology*. The typical alienist is concerned with the study and treatment of mental disorders (technically called psychoses); the clinical psychologist, on the other hand, is concerned *particularly* (though not solely) with the study of *plus* and *minus deviations* from normal mentality. The alienist works chiefly with adults, the clinical psychologist with children. Few alienists possess any expert knowledge of the literature bearing on child or educational psychology, mental deficiency, retardation or acceleration,

stuttering or lisping, special pedagogy or psycho-clinical methods of testing. An alienist accordingly is not to be considered a specialist on the mentally exceptional child in the schools unless indeed he has supplemented his general medical and psychiatric education with a technical study of the psychological and educational aspects of the problem. The alienist of the future will certainly have to secure a different preparation from that now furnished in the medical schools, if he is to enter the field of pedagogic child study.

Before me lies the report of the department of medical inspection of a large school system. Six hundred retarded children were examined in this department, which is in charge of an alienist, who, as I am told, is an expert on the questions of *adult insanity,* but who has no specialized preparation in the psychology and pedagogy of the mentally defective child. Of these children 49.7 per cent. are recorded as feeble-minded. Applying this figure to the 6,000,000 retardates of the public schools of the country, we get a total feeble-minded school population of 3,000,000. This figure, it need scarcely be said, is monstrously absurd. It is fully ten times too large. Feeble-mindedness and backwardness in children, it must be said, are distinct problems from mental alienation, and require for their satisfactory handling a specialist on mentally deviating children. A high grade feeble-minded child can not be identified merely by some rule-of-thumb system of intelligence tests. Feeble-mindedness involves more than a given degree of intelligence retardation. At the same time, lest I be misunderstood, it should be specially stated that psychiatry and clinical psychology will be mutually helped by a closer union. Clinical psychology has many important facts and a valuable experimental technique to offer to psychopathology, and psychopathology in turn is able to contribute facts of great value, and more particularly an effective clinical method of examination, to clinical psychology. As the idiogenic conception of the causation of various psychoses wins greater recognition, clinical psychology will become more and more indispensable to the psychiatrist. It is certain that the efficiency of the clinical psychologist will be greatly increased by a study of mental alienation—not a study of texts on psychiatry, but a first-hand study in institutional residence of individual cases. Any one intending to do psycho-clinical work with mentally deficient children certainly should spend a year or two in residence at institutions for feeble-minded, epileptic and alienated children. The clinical psychologist should be prepared to recognize cases of incipient mental disorder, so that he will be enabled to select these cases and refer them to a psychiatric specialist for further examination.

2. Clinical psychology is not *neurology.* There are important neurological aspects involved in the study of mentally exceptional children. Mental arrest can be largely expressed in terms of neurological arrest, and a clinical psychologist should have a first-hand knowledge of nerve signs and a practical acquaintance with the methods of neurological diagnosis. His knowledge of neurology should be sufficient to enable him to pick out suspected nervous cases and refer them for expert examination by a neurologist. However, it must be emphasized that neurology touches only one side—though an extremely important side—of the problem of exceptional mentality.

3. Clinical psychology is not synonymous with *general medicine.* The average medical practitioner certainly knows far less about the facts of mental variation in

children than either the psychiatrist or neurologist or even the class-room teacher. This fact should occasion no surprise when it is stated that the study of psychology as a science has been practically ignored in the medical curricula throughout the country. The clinical psychologist, however, as I have already said, should be able to detect the chief physical defects found in school children, so that if the laboratory of the clinical psychologist assumes the function of a clearing house for the exceptional child he may be able to refer all suspected cases to proper medical clinics for expert examination and treatment.

4. Clinical psychology is not *pediatrics*. To be sure, the pediatrician deals with children. But his attention is focused on the physical abnormalities of infants; his interest in the phenomena of mental exceptionality is liable to be incidental or perfunctory. In fact, one may read some texts on pediatrics from cover to cover without so much as arriving at a suspicion that there is a body of unique facts converging on the phenomena of departure from the limits of mental normality which require intensive, specialized, expert study and diagnosis. So far as the physical ailments or disabilities of young children are concerned the pediatrician is in a position to render valuable service to the psychoclinicist; likewise so far as concerns the mental deviations of children the psychoclinicist is able to render valuable aid to the pediatrician. But one must not confuse pediatrics with clinical psychology.

5. Clinical psychology is not the same as *introspective, educational or experimental psychology*. It differs from these in its method, standpoint and conceptions. While the clinical psychologist should be grounded in introspective and, especially, experimental, educational and child psychology, expertness in these branches of psychology does not in itself confer expertness in practical psycho-clinical work. Such expertness comes only from a technical training in clinical psychology and from a first-hand prolonged study by observation, or experiment, or test of various kinds of mentally exceptional children, particularly the feeble-minded, the epileptic and the retarded. The skilled specialist in experimental or educational psychology or experimental pedagogy, is no more qualified to *clinically* examine mental cases, than is the skilled zoologist, physiologist or anatomist able to *clinically* examine physical cases. Clinical work, both in psychology and medicine, requires clinical training. The assumption that any psychologist or educationist is qualified to do successful psycho-clinical work, after learning how to administer a few mental tests, is preposterous and fraught with the gravest consequences. Clinical psychology can have no standing in the professions as long as we permit this absurd notion to prevail.

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PENSIONS IN THE ROCKEFELLER INSTITUTE

Pensions for its members and associate members have been provided by the governing boards of the Rockefeller Institute for Medical Research, and have been financially secured by the generosity of Mr. John D. Rockefeller, who has with this purpose in view increased the endowment of the institute by a gift to it of securities amounting to about $500,000 in value. The pension rules which have been adopted provide three-quarters-pay pensions for members of the institute retiring at the age of 65 after fifteen or more years of service, and pensions of from one half to three quarters of full pay, according to the length of service, for members and associate members who retire at 60 years of age. There is also a provision for total disability after ten

years of service, and for widows and orphaned children, at one half the scale upon which members of the staff are pensioned.

SCIENTIFIC NOTES AND NEWS

AT its commencement exercises last week Columbia University conferred its doctorate of science on Colonel William C. Gorgas, Assistant Surgeon General U. S. A., and Dr. Alexis Carrel, Rockefeller Institute for Medical Research.

THE Medical Society of London, at its meeting on May 19, elected Sir David Ferrier, F.R.S., president, in succession to Sir Watson Cheyne.

DR. V. F. K. BJERKNES, professor of geophysics in the University of Leipzig, and Dr. Hugo Hergesell, president of the International Commission for Scientific Aëronautics, Strasburg, have been elected honorary members of the Royal Meteorological Society.

PROFESSOR GEORGE F. SWAIN, professor of civil engineering in the Graduate School of Applied Science at Harvard University, has been chosen chairman of the Boston Transit Commission to succeed the late George G. Crocker.

PROFESSOR CHARLES J. SMITH, for forty-three years head of the department of mathematics in Western Reserve College and Adelbert College, has presented his resignation and will retire from the faculty at the close of the present college year. He served as professor of mathematics and Perkins professor of natural philosophy and astronomy in Western Reserve College from 1870 to 1882, and as professor of mathematics in Adelbert College from 1882 to the present.

ASSISTANT PROFESSOR J. E. READHIMER, superintendent of experiment fields at the University of Illinois since 1901, has entered on his duties as agricultural adviser in Kane County, Illinois. Professor Readhimer began his work at the university when there were no experiment fields in the state outside of Urbana. He has had a part in a great agricultural development in Illinois. As superintendent he has been intimately connected with the development of the experiment fields. He has seen them increase from one (in Urbana) to thirty-five, scattered over the entire state.

GARDNER C. BASSET, Ph.D., of the Johns Hopkins University, in psychology (1913), has been appointed research assistant in the Eugenics Record Office of the Station for Experimental Evolution at Cold Spring Harbor, and will continue the work on the intelligence of inbred white rats begun at Johns Hopkins.

DR. HARRISON J. HUNT, of Island Falls, Maine, has been appointed surgeon to the Crocker Land Expedition which is leaving for the north polar regions on July 2 next, under the auspices of the American Museum of Natural History, the American Geographical Society and the University of Illinois. Dr. Hunt is a graduate of Bowdoin College and medical school and has had eight years' private practise.

THE C. M. Warren committee of the American Academy of Arts and Sciences has increased to two hundred dollars the award to Professor E. W. Washburn, University of Illinois, for his work on the design of an adiabatic calorimeter. Professor H. B. Byers, University of Washington, has recently published two papers on the passivity of iron, dealing with researches which were in part made possible by grants from the Warren fund. This fund was bequeathed to the academy "for the encouragement and advance of research in the science or field of chemistry" and the committee in charge is glad to know of investigations which may properly be aided by it. Professor H. P. Talbot, Massachusetts Institute of Technology, Boston, Mass., is chairman of the committee.

DR. F. D. BARKER, of the department of zoology of the University of Nebraska, has been voted a grant by the trustees of the Bache fund to enable him to continue his study of the parasitic fauna of the Bermudas. Dr. Barker will spend the latter part of this summer and next summer collecting material at Bermuda.

Dr. A. C. Abbott, Pepper professor of hygiene and bacteriology, University of Pennsylvania, has been appointed a delegate to the fourth International Congress of School Hygiene, to be held at Buffalo, August 25 to 30, next, to represent the University of Pennsylvania and the American Physiological Society.

Mr. N. E. Hansen, secretary of the South Dakota State Agricultural Society, has started on his fourth expedition to Siberia, to gather seed of the Siberian alfalfas which he brought over in 1906 for the first time.

Professor B. E. Livingston will spend the summer at the Desert Laboratory of the Carnegie Institution, at Tucson, Arizona, where he may be addressed from June 20 to September 20.

Professor W. A. Henry, emeritus professor of agriculture at the University of Wisconsin, and for fifteen years dean of the College of Agriculture, is again at the university after a five months' absence. He is planning the preparation of a new book on agriculture.

Dr. E. C. Schroeder, pathologist of the Bureau of Animal Industry, delivered the concluding lecture of the annual series under the auspices of the Ohio State University chapter of the Society of Sigma Xi upon the topic, "Relation of Animal to Human Tuberculosis."

Dr. Oscar Riddle, of the Carnegie Institution, lectured on May 5 under the auspices of the Cornell Chapter of Sigma Xi on "A Relation between the Storage Metabolism of Ova and the Experimental Control of Sex."

The Croonian lecture before the Royal Society was delivered by Dr. Robert Broom, keeper of vertebrate paleontology in the South African Museum, on June 5. The subject was "The Origin of Mammals."

The mathematical works of the late Henri Poincaré are to be published by the firm of Gauthier-Villars, under the auspices of the minister of public instruction and the Paris Academy of Sciences.

Professor Francis T. Havard, associate professor of mining and metallurgy at the University of Wisconsin, died at Madison recently from pneumonia. He was thirty-five years of age and was a native of Australia. He attended universities in Australia and Germany and later had charge of mining interests in South America, Africa and Montana. He became associated with the University of Wisconsin in the fall of 1909.

Professor Heinrich Weber, of Strasburg, died of apoplexy on May 17. He is principally known for his profound work in algebra and at various times was rector of the universities of Koenigsberg, Marburg and Strasburg. His wife, who died a few years ago, translated Poincaré's "La valeur de la science" into German.

The regular annual meeting of the American Chemical Society will be held in Rochester, N. Y., September 9–12, inclusive.

A party of advanced students from the department of geology of the University of Illinois, under the leadership of Dr. John L. Rich, left on June 12 for a two-weeks' geological and geographical excursion through neighboring states. The party planned to proceed east to Richmond, Indiana, and Hamilton, Ohio, where a study will be made of the physiography of the district devastated by the recent floods of the Miami River, thence to Cincinnati. From Cincinnati the party will proceed south into the blue grass district of central Kentucky, west to Mammoth Cave, where a stop of two days will be made, and then return to Urbana through Kentucky and Indiana.

On May 28 an informal meeting was held in the Peabody Museum, Harvard University, and the sod was turned to begin the building of the addition to the Peabody Museum which will complete the entire University Museum as originally planned. A short address written by Professor F. W. Putnam was read by Dr. Charles Peabody. Professor Putnam, unfortunately absent on account of illness, recalled the first ceremony in connection with the beginning of the museum when Governor Banks cut the sod, Professor Louis Agassiz turned it over and Mrs. Agassiz put it into the wheelbarrow. In the present exercises

President Lowell cut the sod which was lifted by Mrs. Henry L. Higginson, a daughter of Professor Louis Agassiz and a sister of the late Professor Alexander Agassiz, '55. After this, George R. Agassiz, '84, and Maximilian Agassiz, '89, followed in turn, as well as a number of other officers of the different departments of the University Museum. The money for the addition has been raised by contribution from friends interested not only in the Peabody Museum, but in the University Museum. The building will be pushed forward with energy and it is hoped that the new space for the collections will be available in the course of nine or ten months.

THE tenth annual session of the Puget Sound Marine Station will convene at Friday Harbor, Washington, on June 23, and will continue for a period of six weeks. The courses to be offered will be as follows: algology, R. B. Wylie, University of Iowa; plant ecology, A. R. Sweetser, University of Oregon; elementary botany, William Moodie, Washington State Normal; elementary zoology, H. B. Duncanson, Nebraska State Normal; general ecology, H. S. Brode, Whitman College; embryology of invertebrates, Wm. J. Baumgartner, University of Kansas; ichthyology, E. V. Smith, University of Washington; advanced ecology, Trevor Kincaid, University of Washington; plankton, John F. Bovard, University of Oregon. Facilities will also be offered for research along botanical and zoological lines. The systematic survey of the local fauna which has been in progress for several seasons will be continued by further deep-water exploration. The director of the station, Professor Trevor Kincaid, of the University of Washington, will be glad to give more extended information to persons planning to visit the laboratory.

UNIVERSITY AND EDUCATIONAL NEWS

MR. ANDREW CARNEGIE has undertaken to provide a million dollars for the medical department of Vanderbilt University. Of this sum $200,000 would be given the university immediately for the erection and equipment of laboratories. The income from the remaining $800,000 would be paid annually for the support of the department through the Carnegie Corporation. A condition of the donation provides that the direction of the educational and scientific work of the department be committed by the board of trustees to a small board of seven members, three of whom shall be eminent in medical and scientific work.

MESSRS. JAMES B. and BENJAMIN N. DUKE have given $800,000 more to Trinity College in North Carolina. The college thus met the $150,000 promised by the Rockefeller Foundation and has added one million dollars to its endowment.

GOVERNOR SULZER has signed a bill appropriating $250,000 for a building for the State College of Agriculture at Syracuse University. Plans for the building are in the hands of the state architect and ground for the building will be broken early in the summer. The building will be located on the western end of the university campus, and when completed will be the largest and best equipped forestry building in the United States. Provision will be made in the basement of the building for laboratories for timber-testing and for investigations in the production of paper pulp and in the destructive distillation of timber. That is, there will be in a simple and miniature way complete paper-making and acid plants. With this will be a very complete wood-working shop where students may get acquainted with woods from the builder's standpoint. Besides offices, class-rooms and laboratories, there will be an auditorium on the third and fourth floors with a seating capacity of 1,000. Such closely related lines as forest botany, forest zoology and forest entomology will be taken care of in especially equipped laboratories.

THAT the University of Wisconsin has 5,970 students at Madison this year and 5,523 enrolled in correspondence-study courses—a total of 11,493—is shown by the new catalogue of the university which came from press this week. Every college in the university except the College of Engineering shows a gain. The College of Agriculture, with a gain of 108 students over last year's enrollment of 743,

making this year's total enrollment 851, shows the largest increase of any division of the university. The College of Letters and Science has an enrollment of 2,528, as compared with 2,504 last year. The course in pharmacy in this college has increased from 38 to 44, and the Medical School from 57 to 66. The course for normal school graduates has a registration of 66 as compared with 58 last year. The course in commerce has 335 students, a loss of 5, while the course in chemistry has 77 students, a loss of 11. The Law School with 167 students has 9 more students than last year. The Library School has 36 students, two more than last year, and the largest number that can be accommodated with the present facilities. The School of Music has a total of 418 students. The College of Engineering has 678 students, a loss of 50. This loss is largely in the present freshman class, which has 49 students less than that of last year. The Graduate School this year numbers 394, a gain of 17. The faculty of the university consists of 624 members, of whom 95 are full professors, 45 associate professors, 111 assistant professors, 21 special lecturers, 201 instructors and 152 assistants.

John L. Ulrich, Ph.D. (Johns Hopkins University '13), has been appointed instructor in physiological psychology in the Catholic University of America.

Dr. W. J. V. Osterhout has been promoted to be professor of botany at Harvard University; Dr. P. W. Bridgman has been promoted to be assistant professor of physics.

Dr. B. M. Allen, assistant professor of anatomy in the University of Wisconsin, has been chosen by the board of administration of the University of Kansas to be head of the department of zoology, succeeding Professor C. E. McClung, who a year ago became head of the department of zoology in the University of Pennsylvania. Promotions have been made as follows: F. H. Billings, from associate professor to professor of bacteriology; N. P. Sherwood, from instructor to assistant professor of bacteriology; W. H. Twenhofel, from assistant professor to associate professor of geology; T. T. Smith, from instructor to assistant professor of physics; R. K. Yodng, from instructor to assistant professor of physics and astronomy; W. J. Baumgartner, from assistant professor to associate professor of zoology; F. C. Dockeray, from instructor to assistant professor of psychology; H. W. Josselyn, from assistant professor to associate professor of education; H. A. Rice, from associate professor to professor of civil engineering; F. H. Sibley, from assistant professor to associate professor of mechanical engineering.

The corporation of the Massachusetts Institute of Technology, at its meeting on June 6, confirmed the following promotions and appointments in the instructing staff of the institute: Dr. J. Arnold Rockwell appointed medical adviser in place of Dr. F. W. White. Associate Professor F. A. Laws, promoted to professor of electrical engineering. The following assistant professors are promoted to the grade of associate professor: Earle B. Phelps, research in chemical biology; S. P. Mulliken, organic chemical research; M. S. Sherrill, theoretical chemistry; G. E. Russell, civil engineering; Ervin Kenison, drawing and descriptive geometry; N. R. George and L. M. Passano, mathematics; M. deK. Thompson, electrochemistry; L. E. Moore, civil engineering. Two instructors were promoted to the grade of assistant professor: Edward Mueller, inorganic chemistry; J. W. Howard, topographical engineering. A number of assistants have been promoted to the grade of instructor, namely, N. S. Marston, electrical engineering; Duncan MacRae, inorganic chemistry, replacing C. R. Cressy; Dean Peabody, mechanical engineering; W. H. Wengert, mechanical engineering; W. J. Murray, analytical chemistry, replacing Mr. Fallon, and C. K. Reiman, inorganic chemistry. Assistant J. M. Livingston has been made research associate in applied chemistry. The following are new appointments to assistantships: Edward A. Ingham, biology; Robert D. Bonney, Warren E. Glancy, Leon W. Parsons and Charles S. Venable (half time), analytical chemistry, replacing E. T. Marceau, P. M. Tyler, C. K. Reiman (pro-

moted) and J. A. Gann, respectively; F. W. Lane and Philip B. Terry, organic chemistry, replacing W. J. Murray and J. W. Livingston, promoted; Arthur E. Bellis and Charles L. Burdick, theoretical chemistry, replacing B. F. Brann and Duncan MacRae, promoted; Lester F. Hoyt, water analysis, replacing W. J. Daniels; Francis H. Achard, Henry C. Harrison and Russell E. Leonard, electrical engineering, replacing D. M. Terwillinger, J. P. King and H. G. Jenks; Edgar W. Taft, military science, replacing A. J. Pastence; John P. Constable, naval architecture, replacing R. B. Pulsifer; Warren K. Green and William G. Horsch, physics, replacing Mr. Wells and Mr. Wilkins; Millard W. Merrill, electro-chemistry, replacing Mr. Gonzales; R. G. Daggett, research assistant in sanitary chemistry; George Richter and W. B. Van Arsdel, research assistants in applied chemistry; H. F. Thomson, assistant to the director of the research laboratory of electrical engineering and part-time instructor in electrical engineering; Robert E. Rogers, instructor in English; Thomas S. Holden, part-time instructor in mathematics; Clarence Hale Sutherland, instructor in civil engineering, replacing Mr. Bradbury, resigned; Ferdinand H. Pendleton, Jr., assistant in technical analysis, replacing Mr. Bishop.

DISCUSSION AND CORRESPONDENCE

THE CONSTITUENCY OF THE EXPERIMENT STATION

IN a communication to SCIENCE for May 9, page 708, Dr. Raymond Pearl, after stating that theoretically it is a primary function of the state experiment stations to conduct researches of a fundamental character which shall be calculated to discover basic natural laws, says:

> Actually, with a few rare and partial exceptions, experiment stations do nothing of the sort. On the contrary, what they do engage in is experimental work of a kind carefully calculated to make as strong an appeal as possible on the basis of its supposed "practicality" to the scientifically uneducated and uncritical farmers who make up its constituency. The experiment-station investigator in many cases (though happily not in all, as I am able personally to affirm after five years' experience in Maine) is compelled by force of circumstance over which he has no control to supplicate the great goddess truth with one ear closely applied to the ground in order that he may catch the first and faintest murmur of "what the public wants." If he has the temerity to venture upon a piece of research for which by the most extreme sophistry no evidence of immediate practicality can be adduced, he must do the work sub rosa and publish the results in such place that by no possible chance can the constituency ever learn of it.

It would be impossible to pack into the same space a greater amount of error and unwarranted sarcasm than is contained in these words. We are glad that Maine is so shining an exception to the general deplorable conditions herein set forth, and the state is to be congratulated upon its possession of so brilliant an exponent of what agricultural research ought to be. When, however, he writes in this sweeping fashion regarding the conditions surrounding some forty other stations in states covering a continent, many if not most of them some thousand or more miles away, it is not strange that in evolving the material largely from his inner consciousness, he has succeeded admirably in describing what the stations are not.

I write to discuss only a single sentence of this quotation and to resent its import in justice to a constituency not likely to answer for itself in these columns.

Dr. Pearl speaks of "the scientifically uneducated and uncritical farmers" as making up the constituency of the experiment stations and as constituting a real and natural bar to high-grade work. This is a sweeping and serious indictment against a series of federal institutions working in and supported by all the states of the union.

No special superiority is claimed for Illinois, but this occasion is taken to point out that of thirty-five farmers of the state serving on the advisory boards of our experiment station I happen to know of ten who are college graduates representing the following institutions: Dartmouth, Amherst, Yale, Illinois, Iowa and Cornell. How many of the

other members of these boards are college men I do not know, as this information is merely accidental, but they constitute a goodly company. It is the judgment and the criticism of these men that this station must first reckon with, and I have found them entirely competent to judge of the scientific reliability and soundness of our experiments as well as of their practical significance; nor is it necessary to publish sub rosa the better product of our laboratories.

These men are selected not by the station itself, but by the farmers' organizations of the state, and it is fair to assume that as time goes on the men nearest the station and its work will be college trained. In a few days this university will graduate nearly a hundred men from a four years' scientific course in agriculture. By the ratio of the past, fifty-five per cent. of these graduates will go at once to their farms, where, with even a larger number of men who have not fully completed the course and with a still larger number graduated from other colleges, they will constitute a rapidly increasing constituency trained not only in agriculture as a business, but in scientific methods of study. It is this class of men and the colleges which trained them that are in truth covered by what Dr. Pearl denominates as a scientifically uneducated and uncritical constituency.

In this state, and in others so far as my observation goes, since the station started a quarter of a century ago, the uneducated and uncritical farmers simply ignore the station and all it says. They do not count one way or the other in its policy, nor do they constitute or even characterize its constituency.

E. Davenport
University of Illinois

Does a Low-Protein Diet Produce Racial Inferiority?

To the Editor of Science: The argument usually advanced against the adoption of the "low-protein" diet plan is that the races practising it, when compared with those employing a protein-rich dietary, show in general some points of physical inferiority or lack energy, aggressiveness or courage. To this it has been replied that the diet is the result, rather than the cause, of such racial characteristics, but by the writers of most books on nutrition and related subjects, both scientific and sensational, this reply has been either overlooked or regarded as inadequate.

As there are no doubt many people who could be benefited by the adoption of a low-protein diet, judging from the results of the studies by Chittenden and others, but who hesitate to try it because of uncertainty as to its possible unfavorable effects, it would seem that the results of new investigations bearing upon the matter should be given wider publicity than their publication in the technical journals usually affords. Accordingly, the present note is offered to call attention to recent work which apparently tends to weaken the above argument in the cases of two of the countries often used as examples of its application, namely, Japan and India.

The disease called beri-beri, a multiple inflammation of the nerves, which is prevalent in most oriental countries, though also known among the fishermen of Newfoundland and Labrador, but which has been most extensively studied in Japan, long baffling explanation, has frequently been brought forward as evidence of the insufficiency of the low-protein diet. The cause of the disease has now been discovered, and found to have no relation to the deficiency of the diet in proteins in the usual sense. For some time it had been known that the use of unhulled grains instead of those from which the bran was completely removed would prevent the disease, and further, that the bran itself contained some principle which would cure many cases that had not progressed too far, but these facts, based chiefly on experiments on animals, were discounted by those who wished to uphold the protein theory.

Finally, however, three Japanese investigators[1] have succeeded in isolating from rice

[1] Suzuki, Shamimura and Odake, *Biochemische Zeitschrift*, XLIII., 89.

bran a nitrogenous base, which they call *oryzanine,* which proves to be a specific for the cure of the disease. Pigeons, chickens, mice and dogs, which would die in a few days or weeks with symptoms of starvation and nerve inflammation if fed on polished rice alone, remained in health when small amounts of oryzanine were added to the diet, as little as 0.3 gram per day being required for an average-sized dog. The bran of other grains and most vegetables were found to contain oryzanine, or at least a substance with similar therapeutic action, while milk, eggs, fish and meat showed little or none, although the alcoholic extract of fresh meat had some beneficial effects on dogs. The authors state that the experience of the Japanese in regulating the dietary of their navy, for instance, confirms in every way the results of these studies, in that beri-beri was stamped out only when foods rich in oryzanine were introduced, and feel justified in concluding that this substance is an absolute necessity for the maintenance of the health, not only of the lower animals, but also of the human race.

Many inferences might be drawn from these results. They show, for instance, the reason why graham bread is more desirable than white, which has been urged by diet reformers for many years, yet has been repeatedly questioned by scientists because no definite reason could be given and because actual experiment showed the white flour product to possess the greater digestibility. They suggest, indeed, that certain nervous troubles, which afflict the civilized races in general and the United States in particular, may have as at least a contributing cause the extensive use of grains from which the bran has been removed. But most interesting of all they show that the apparent inferior power of the Japanese and other eastern races to resist such diseases as beri-beri has nothing whatever to do with their low-protein diet as such, but is caused by their following the dictates of fashion and making use of bran-free, polished rice, as their staple article of diet.

The evident inferiority of the races inhabiting India, which enables a mere handful of British soldiers to keep them under control, is very often referred to as evidence of the inadequacy of the vegetarian—essentially low-protein—dietary made use of by these peoples, as a matter of religious observance. The recent survey by the Rockefeller Sanitary Commission has shown, however, that from 60 to 80 per cent. of the inhabitants of that country are infected with the hookworm. And as the degenerating influence of this parasite on both the physical and intellectual development of its victims is now so well known that further discussion of it is unnecessary, it would appear that we have herein sufficient explanation of the status of these races, without being obliged to assume that their diet is faulty.

With two of the supposedly most typical illustrations of the unfavorable results of a deficiency of protein in the dietary thus explained away, we are surely justified in inquiring, is there any evidence whatever that a low-protein diet ever causes or aids in the production of racial inferiority?

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A RULING THAT IS AGAINST THE RULES

IN the "Opinions rendered by the International Commission on zoological nomenclature"[1] *Opinion 11* (p. 17) reads as follows:

> The "Table des genres avec l'indication de l'espèce qui leur sert de type" in Latreille's (1810) "considerations générales," should be accepted as designation of types of the genera in question (Art. 30).

The matter concerns the meaning of the word *type,* as used by Latreille. Some authors hold that Latreille could not have used it in the modern sense of *genotype,* simply because that particular meaning was entirely unknown at this time. This view surely is supported by common sense.

But admitting that there was cause for con-

[1] Smithsonian Inst. Publ. 1938, July, 1910.

tention, and that the case was not fully covered by the regular international rules on nomenclature, it is clear that it might be settled in one of two ways: either by making a special ruling with regard to it, or by inserting a paragraph in the general set of rules, which would cover it.

Both ways have been used: the first is the ruling given in opinion 11; the other is the second paragraph in Art. 30, II. (*g*), in the "International Rules of Zoological Nomenclature," as published in the Proceedings of the Seventh International Zoological Congress, 1912, p. 46. This says:

> The meaning of the expression "select the type" is to be rigidly construed. Mention of a species as an illustration or example of a genus does not constitute a selection of a type.

Every one familiar with the case knows that this paragraph was added to the rules with the special purpose of disposing of the doubt as to the meaning of Latreille's word *type*. At any rate, I know of no other case where it might be applied.

The two decisions are contrary to each other. The ruling made in opinion 11 accepts Latreille's "types" as genotypes in the modern sense. The paragraph under Art. 30, quoted above, forbids it to accept them as genotypes. For there is not the slightest question that Latreille meant the word *type* in the sense of illustration or example, for the other sense did not exist at that time. The argument (opinion 11, p. 18) that the use of the definite article (l'espècè) indicates that it was meant in the latter sense, is simply preposterous, since by substituting "une espècè" for "l'espècè" the sense of the sentence would not be changed at all.

It is much to be regretted that such an absurd situation has been created. Of course, this might be excused, since the opinion 11 was published two years ahead (in 1910), while the amendment to Art. 30 of the rules did not appear in print till 1912. Yet it might have been expected, for obvious reasons, that the latter should have been known to all members of the International Commission on nomenclature as early as 1907.

Of course the paragraph of the regular rules should prevail. But in order to remove all doubt in the minds of zoologists not familiar with the facts, and in order to avoid that the rulings of the commission might become a farce, one of the next "opinions" to be published should reverse opinion 11. But whether it is expressly repealed or not, opinion 11 can not stand any more, and zoologists not conforming to it should not be criticized for it.

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SCIENTIFIC BOOKS

The Omaha Tribe. By Alice C. Fletcher, holder of the Thaw fellowship, Peabody Museum, Harvard University, and Francis La Flesche, a member of the Omaha tribe. Twenty-seventh Annual Report of the Bureau of American Ethnology, 1905–06. Washington, Government Printing Office. 1911. Pp. 672, plates 65, Figs. 132.

The most obvious thing about this monograph is the authors' well-nigh complete neglect of the work of their predecessors. It is their avowed purpose (p. 30) to borrow nothing from other observers and to present "only original material gathered directly from the native people." Apart from any considerations of historical justice, this principle is unjustifiable from the standpoint of the student. A work so ambitious will naturally be regarded by almost every reader as definitive, as embodying everything that is known concerning the ancient life of the Omaha and as taking cognizance of all additional and contradictory testimony. In both hopes he will be disappointed. There are subjects on which other observers have collected information not furnished by Miss Fletcher and Mr. La Flesche. The parent-in-law taboo, for instance, is treated more fully in Say's notes[1] and in J. O. Dorsey's classical work[2] than in the brief

[1] In James's "Account of an Expedition from Pittsburgh to the Rocky Mountains" (London, 1823), I., pp. 232–234.

[2] "Omaha Sociology," Third Ann. Rept. Bur. Eth., pp. 262–263.

paragraph of the volume before us (p. 335). Say, indeed, furnishes admirably illustrative concrete data. Why should these be withheld from the reader? Even where no additional information is given by an older writer, it is often very important to know the earliest time at which the mere existence of some custom has been recorded. This is especially true of the Plains area where so much diffusion has demonstrably occurred. Yet the authors feel at liberty to ignore the fact that the office of "crow"-wearing policemen for the buffalo hunt was noted by Say[3] as early as 1820. Again, few subjects have aroused more interest among American ethnologists than age-societies and military organizations. J. O. Dorsey's data[4] on these are meager enough, but the authors have practically not a word on either.

The neglect of contradictory evidence gathered by others constitutes a still more serious defect, because the unwary reader thus obtains a one-sided, unduly simplified picture of the condition of affairs. According to Miss Fletcher and Mr. La Flesche, the Black Shoulder gens had two subgentes, one of which was still further subdivided. We get no suggestion that there was any conflict of opinion among their informants or that any change may have occurred in relatively recent times. Additional names, however, are given by J. O. Dorsey,[5] who carefully recounts the contradictory statements of his native authorities and hints at recent changes in the subgentes. With reference to two gentes the authors state that lesser groups within these units "have been mistaken for subgentes" (pp. 172, 178). Had notice been taken of J. O. Dorsey's data, it would be possible to understand what may be meant by these words. As it is, we may assume with some plausibility that the phrase is a covert criticism of J. O. Dorsey, for that writer undoubtedly does speak of "subgentes" where his successors find only "groups." However, a reference to Dorsey's text[6] and to the authors' definition of a "subgens" reduces the criticism to a verbal misunderstanding. Miss Fletcher and Mr. La Flesche (p. 137) understand by "subgens" a section of the gens that has a distinctive rite, while a "subdivision" or "group" has none, though it had a particular office in the rite belonging to the gens. Dorsey does not understand by "subgens" anything of the kind. He tells us that in his opinion two of his main informants always mean a classification for marriage purposes when they speak of divisions of a gens, and it is clear that this feature is uppermost in his own mind whenever he uses the term "subgens." If the authors' criticism is meant as an innuendo against Dorsey, it is not only disingenuous but incorrect.

It is very interesting to examine the solitary instance of open criticism directed by the authors against their great predecessor. They write (p. 589, footnote):

> The statement has been made (11th Ann. Rep. Bur. Ethnol., 542), "In two of the buffalo gentes of the Omaha (the Iŭke-sabĕ and Hañga) there is a belief that the spirits of deceased members of those gentes return to the buffaloes" and the buffalo is spoken of as "the eponymic ancestor." The writer here cited fell into the error of regarding the animal which furnished the peculiar symbol in the rites of these kinship groups as the progenitor of the members of the groups. No such confusion seems to have existed in the Omaha mind. Men were not believed to be descended from animals. If the expressions "Buffalo people," "Elk people," "Deer people" or "Thunder people" were used, these descriptive terms were not employed in a literal sense but as tropes.

A little farther (p. 601) we read a still more categorical denial:

> Although, according to the Omaha view, man is so closely connected with the animals, he was not born of them; no trace has been found showing any confusion or mixture of forms; no Omaha believes that his ancestors were elk, or buffalo, or deer, or turtle, any more than that they were the wind, the thunder or the sky.

This criticism, whatever be its merits, is

[3] *Op. cit.*, p. 189.
[4] *Op. cit.*, pp. 342, 352.
[5] *Ibid.*, pp. 230–231.
[6] *Op. cit.*, pp. 242, 245, 258.

dishonest in selecting for its point of departure an incidental statement in a work not specially devoted to the Omaha, instead of taking the fuller accounts in the "Omaha Sociology."[7] In this paper there is given the translation of the words employed in addressing a dying Iñke-sabe, and these words certainly imply a belief that the person addressed is going to rejoin his ancestors, the buffalo. On the same page Dorsey also cites a legend in which the Iñke-sabe are said to have been at one time buffalo. If Dorsey's critics wish to say that he has mistranslated his informant's words, they should so state. If they wish to say that he has sucked his information out of his thumbs, they should so state. If they wish to say that his informants did not represent the consensus of Omaha opinion, they should so state. They should state further that Dorsey himself does not hesitate to tell us that the legend was unknown to two of his best informants. Dorsey was ahead of his generation and of some of the succeeding generation in not suppressing data that might interfere with the smoothness of his tale.

"The Omaha Tribe" must, however, be considered primarily as a field report. Judged from this point of view, it will be found wanting in several respects. In the first place, the tremendous wealth of concrete material is classified according to canons of aboriginal rather than of scientific logic. Thus, the Medicine Pipe ceremony is described under the heading of Music, apparently because its native name means "to sing for some one." Data on agriculture and hunting are assembled with those on the ritual of the maize and the ceremonial hunt. "Social Life" is made to include not only such legitimate topics as kinship terms, courtship and marriage, and etiquette, but also cooking and foods, dressing and tanning skins, quill work, weaving, personal adornment and clothing.

Secondly, there are large fields of ethnological interest that the authors either do not touch at all or treat in a very unsatisfactory manner. Foremost among these is mythology and folklore. Inconsistent as such a supposition is with the authors' general attitude, we are tempted to assume that their failure to enter into these subjects is an expression of their admiration for the thoroughness with which J. O. Dorsey has accomplished the task that was to be done in this field.[8] Unfortunately the reader does not profit by this exhibition of tacit generosity. For all he could learn from Miss Fletcher and Mr. La Flesche, there has never been published any systematic collection of Omaha tales. Moreover, the fact that a splendid collection exists does not absolve monographers of a tribe from the duty of briefly characterizing the native mythology with reference to both substance and form and of showing its relations to other mythologies. The few paragraphs devoted to this matter in the volume before us (pp. 600, 601, 608) can not be regarded as even a serious attempt in this direction.

Another very remarkable deficiency appears in the discussion of material culture and art. The fact that these subjects are treated with disproportionate brevity is a venial fault, for there are few monographs in which all phases of culture are treated with uniform thoroughness, and some allowances must be made for individual interests. But every professional ethnologist may reasonably be expected to pay some attention to points that have come to be of theoretical interest to his fellow-students. Many questions of this sort relating to the material culture of the Plains Indians have been indicated by Dr. Wissler,[9] but very few of them are elucidated by the authors. We do not learn anything of the form of the travois mentioned on page 275; the description of the cradle-board (p. 327) is too vague for comparative purposes; no opinion is expressed as to the antiquity of the men's shirt among the Omaha (p. 355). As the distribution of painted and embroidered patterns has been diligently studied among the Plains Indians for at least ten years, and as Kroeber

[7] *L. c.*, pp. 229, 233.

[8] "The Cegiha Language," Contributions to North American Ethnology, Vol. VI.

[9] "Material Culture of the Blackfoot Indians," Anthrop. Papers Amer. Mus. Nat. Hist., Vol. V., Pt. 1.

and Wissler have attempted to characterize the art of several Plains tribes, the failure of the authors to furnish the data necessary for classifying the Omaha with reference to their decorative designs is even more surprising.

The subjects that have particularly appealed to the authors are sociology and ceremonial life. The chapter on Tribal Government constitutes a real contribution, giving a clear outline of the several grades of chiefs, the council of seven chiefs and the modes of election. The discussion of the Sacred Pole is also a creditable performance, though it adds rather details than anything fundamental to our previous knowledge. The analogies pointed out by J. O. Dorsey between the Hedewatci and the Sun Dance stand confirmed by the new evidence (p. 253). Unfortunately, in these chapters, as elsewhere, there appears the tendency, now definitely abandoned by ethnologists, of attaching historical value to the origin accounts of a primitive tribe, in spite of their naïvely rationalistic psychology. Though the authors seem to regard the establishment of the chiefs' council as "a development of earlier forms rather than an invention or arbitrary arrangement of the 'old men'" (p. 207), other passages clearly reveal the antiquated view just criticized. In the section on Tribal Organization (p. 134 ff.) we are actually asked to believe that the dual division of Omaha society was but the reflection of a mythological conception! This whole section adds very little to our comprehension of the subject. The specific marriage-regulating functions of the subgentes, touched upon but not clearly expounded by Dorsey, remain unilluminated. What is more important, the authors have not logically correlated their own data. We are told that the gens was exogamous (pp. 195, 325); that the subgens or subdivision of a gens was exogamous (p. 137); that there was a tradition as to the exogamous character of the two grand divisions and that "of the marriages in existence among the Omaha twenty-five years ago, a good majority represented the union between members of gentes belonging to the two rather than to one of these grand divisions" (p. 135). Do not the authors recognize the fact that if a gens is exogamous, any smaller group within the gens *must* be exogamous; that if an association of five gentes is exogamous, any one of the gentes *must* be exogamous? The problem is to determine which social group is primarily, and which derivatively, exogamous. This it may not be possible to do at present, but it is at least desirable that professional ethnologists should see the problem.

The terms of relationship (pp. 315–17) are not presented in a satisfactory way, though it is true that the subject is an exceedingly difficult one. Here, where the entire psychological interest lies in the native point of view, the authors take for their starting-point the *English* classification of kin. The consequence is great clumsiness of arrangement and useless repetition of terms. Moreover, some interesting meanings of native terms given by Say are omitted in the present list.

By far the most valuable addition to our knowledge is to be found in the chapter on Societies (pp. 459–581), of which two classes are distinguished—the social and the secret societies. The former include the Hethushka (Grass Dance) organization, membership in which was dependent on the reception of public war honors. Our attention is called to the fact that, while the dance has spread over a wide area, only the Omaha observe the religious rites of the opening ceremony. Another (chiefs') society is said to have been the only one "in which headgear that approximated the character of a mask was used" (p. 481). It would be interesting to know whether these headgears resembled those worn by two officers of the Mandan Bull society,[10] in which the face was covered and eyeslits were provided. According to Dorsey,[11] mere buffalo-skin caps, with the horns standing up and the buffalo hair hanging down below the wearer's chest, were worn by four members of another Omaha organization, the Buffalo Dancers. It is curious that in mentioning the Tokalo (Tukala) performance neither Miss Fletcher

[10] Maximilian Prinz zu Wied, "Reise in das innere Nord-America," II., p. 142.

[11] *Op. cit.*, p. 348.

nor Dorsey should have recognized the name as the Dakota term for "fox" or "kit-fox."

Most of the secret societies were entered by virtue of a dream or vision, those having received a revelation from some particular supernatural power being united in the same organization. This does not, however, apply to the Shell society, into which persons were admitted by unanimous consent of the members. All the offices in the last-mentioned society were obtained by purchase; in the other organizations this element, which plays so important a part among several of the Plains tribes, does not seem to be pronounced. Societies composed exclusively of women have not been found among the Omaha (p. 459), but women were evidently prominent in the Shell society, and tradition states that in early times its principal leader was a woman (p. 516). The Shell and the Pebble societies perform shamanistic practises, of which a "shooting" ceremony common to both recalls the Midewiwin of the Central Algonkian, and the authors incline to the opinion that the two societies are historically related. From the fact that the Pebble rituals deal with "more fundamental conceptions" than the origin myth of the Shell society they infer that, granting the relationship, the Pebble society is the older of the two (p. 529). Indeed, there is evidence that at least one of the elements of the Shell performance, the use of a swan wing, has been borrowed from the Pebble society (p. 519); and the description of the "shell" as a round stone in one of the ritualistic songs (p. 529) is interpreted by the authors as pointing in the same direction. Nevertheless, it is quite possible for one society to have borrowed special features from another without being necessarily of later origin in the totality of its traits. As for the relatively more fundamental character of the Pebble society, the authors do not explain what may be their criterion of greater antiquity or primitiveness. One thing is clear: the comparison of the ritual of one organization with the origin myth of another is unjustifiable, though consistent with the authors' belief that the Shell society was founded upon the myth accounting for its origin (p. 516). Such a view has become less and less tenable as proof has accumulated in different areas that ritual is primary and ritualistic myth secondary.

While the evidence for the greater antiquity of the Pebble society is thus inconclusive, the authors' opinion on the historical connection between the Pebble and Shell organizations seems to me correct. It has been challenged by Radin,[12] but apparently on the basis of a single loose and misleading statement by the authors, viz.,

> As these two societies are the only ones in the tribe which observe shamanistic practises and as they both strongly emphasize magic, it is not impossible that at one time they may have been connected (p. 581).

Radin quite properly objects that the observance of shamanistic tricks is too general a phenomenon to warrant the conclusion and that it is unnecessary to assume any historical connection "unless this has been shown to be the case." The authors certainly should have defined what they meant by "shamanistic," but it seems clear that they do not use the term in the accepted sense but with some specific connotation. For to nearly every one of their secret societies they ascribe what others might call "shamanistic practises," yet these are said to be confined to the two societies under discussion. The authors have obviously arrived at their view because common to both societies are several specific features, such as organization by lodges and the shooting ceremony. These features are not found in the Bear and Buffalo and Ghost societies; they are, therefore, not generally phenomena of Omaha life, and their double occurrence is not explained as a mere reflection of Omaha modes of thought. Whatever may be the origin of other elements of the two ceremonial complexes, the traits mentioned have had, in all probability, a common origin.

The foregoing paragraphs have pointed out with sufficient clearness the character of Miss Fletcher and Mr. La Flesche's book. The au-

[12] "The Ritual of the Winnebago Medicine Dance," *Journal of American Folk-Lore*, XXIV. (1911), p. 191.

thors have placed us under obligation by adding a considerable number of facts to our knowledge of the Omaha, notably on the subject of societies. They have not accomplished the task of giving us a definitive study of Omaha ethnology. We feel grateful for the new data presented by them, but we are also very grateful for the fact that they have had for their predecessor so sane, conscientious and competent an ethnographer as the late Rev. J. O. Dorsey.

ROBERT H. LOWIE
AMERICAN MUSEUM OF NATURAL HISTORY

BOTANICAL NOTES

NOTES ON RECENT BOOKS AND PAMPHLETS

GEORGE F. ZIMMER'S "Popular Dictionary of Botanical Names and Terms" (Dutton) is intended mainly for botanists, horticulturists and others who have to deal much with plant names. The little book of 122 pages is a little different from the usual type of botanical dictionaries, more attention being given to the meanings of specific names, and this will commend it to many students who are somewhat deficient in their knowledge of Latin. For those it would have been well to have at least indicated the accent for each name.

The little books brought out by Dr. Gustav Lindau, of Berlin, under the general title of "Kryptogamenflora für Anfänger" promise to furnish models which might well be followed by American makers of similar books. Already three books have appeared, namely: "Die höheren Pilze"; "Die mikroskopischen Pilze"; and "Die Laubmoose." Bound in substantial cloth, and containing about 250 pages, these books commend themselves to us as admirably adapted for their purpose—namely, that of helping beginners in the systematic botany of the lower plants.

Bergen and Caldwell's "Practical Botany" (Ginn) aims to relate the study of plants in the secondary schools to everyday life more "than is usually done." Accordingly the book is distinctly of the informational rather than the scientific type, and for this reason will appeal to many principals and boards of education. The present reviewer is not in sympathy with the notion that science must always be related to "everyday life" (whatever that may imply), but he finds much to commend in the book. The authors know the science, and pedagogics so well that they have made a useful book, whose faults are due to the underlying theory rather than to any shortcomings on their part. This theory is accountable for the chapters on Timber, Forestry, Plant Breeding, Plant Industries, Weeds, which contain much that is certainly interesting, but that is just as certainly *not botany*. It would be much better for the botanists to allow these applications and extensions of botany to be taken up by foresters, agronomists, horticulturists, agriculturists, etc., a task for which they are entirely competent. We should respect the boundary lines between a science and its applications.

Winkler's "Botanisches Hilfsbuch" (Hinstorff) gives interesting data regarding about twelve hundred plants (mainly tropical) that have economic value. Although primarily designed for tropical planters, merchants, officials and explorers, it will be found to be a useful book in every botanical library.

The Dudley Memorial Volume published by Stanford University contains papers, appreciations and contributions in memory of the late Professor William R. Dudley who died June 4, 1911. In addition to the memorial addresses and papers and lists of Professor Dudley's pupils (covering 32 pages), the volume includes eight scientific papers. The first of these—"The Vitality of the *Sequoia gigantea*"—was prepared by Professor Dudley himself. The others are "The Morphology and Systematic Position of *Calycularia radiculosa*," by D. H. Campbell; "Studies of Irritability in Plants," by G. J. Peirce; "The Gymnosperms growing on the Grounds of Stanford University," by LeRoy Abrams; "The Synchytria in the Vicinity of Stanford University," by James McMurphy; "The Law of Geminate Species," by D. S. Jordan; "Some Relations between Salt Plants and Salt Spots," by W. A. Cannon; "North American

Species of the Genus *Amygdalus*," by W. F. Wight. The volume constitutes a fitting tribute to the botanist whose life it commemorates.

Burman's "Flora of Manitoba," which was printed two years ago, contains in a small 30-page pamphlet a general discussion of the vegetation of the province of Manitoba followed by a list of the species of flowering plants and ferns. It is the only available guide to the plants of that part of Canada.

Stone's "List of Plants Growing without Cultivation in Franklin, Hampshire and Hampden Counties, Massachusetts" (1913), reminds one of the previous classical lists by Hitchcock and Tuckerman which appeared many years ago under similar titles, and dealing with the flora of the Connecticut Valley. It contains 1,493 species of ferns and flowering plants, 1,190 of which are native, the remaining 303 being naturalized.

Meier's "School and Home Gardens," while dealing with plants, is not botanical, though of interest to many botanists. It is designed primarily to help in the commendable effort to interest children in the planting of seeds and the growing of such plants as may be grown in the windows of school buildings or out of doors, under ordinary care. It can be commended most heartily.

Allied to the last is E. Benjamin Andrews's "The Call of the Land" (Judd) dealing largely with out-of-doors, and the things that grow there. While not botanical, it breathes of flowers, and grasses and growing crops, and of the shrubs and trees that make for comfort and beauty and happiness. It is a book distinctly worth while.

A recent number of the *Missouri Botanical Garden Bulletin* includes descriptions of the laboratories in the garden, accompanied by four half-tone plates from photographs. Accompanying the descriptions is a general discussion containing many suggestive sentences, as "Botanical laboratories are the workshops of those who study plants scientifically." "It is to be remembered that the important botanical gardens of the world are educational institutions." "In the broadest sense these laboratories must represent the possibility of using apparatus and chemicals, books and herbarium specimens, live material from garden or field, and cultures of microscopic organisms."

A recent circular (113) of the Bureau of Plant Industry contains a suggestive paper on soil bacteriology, by K. F. Kellerman, in which he shows that it is "a subject of almost bewildering complexity, but very intimately associated with the normal physiology of all crop plants." In a later circular (120) the same author has a short paper on nodule-forming bacteria (*Bacillus radicicola*) which should be helpful to those attempting to inoculate the soil with these organisms.

Recent numbers of the *Botanical Magazine* (Tokyo) contain Makino's "Observations on the Flora of Japan," Matsuda's "List of Plants Collected in Hang-chou" (both in English), and Koidzuma's "Morphology, Systematik and Phytogeography of Cupuliferæ" (in Japanese), with other shorter articles.

Charles E. Bessey
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SPECIAL ARTICLES

PRELIMINARY NOTE ON THE RELATIVE PREVALENCE OF PYCNOSPORES AND ASCOSPORES OF THE CHESTNUT-BLIGHT FUNGUS DURING THE WINTER[1]

In studying the dissemination of the chestnut-blight fungus during the past winter the writers obtained some results that showed that, contrary to the generally accepted opinion, pycnospores are produced in enormous numbers and washed down the diseased trees during every winter rain.

The production of pycnospores was tested by what we have termed pycnospore traps. A part of the rainwater flowing over a canker was conducted down a glass slide and through a mass of absorbent cotton. After each rain the cotton of the traps was brought to the laboratory and a quantitative determination

[1] Investigations conducted in cooperation with Office of Forest Pathology, U. S. Department of Agriculture.

made of the number of viable spores of the blight fungus which were retained by each trap. Traps were set at six different locations at West Chester, Pa., and analyses have been made after each rain period for the months of January to April, inclusive.

There were seven rain periods in January with precipitation varying from 0.13 to 0.88 inch. The number of viable pycnospores obtained from each trap varied from 55,000 to 61,255,000 for each analysis. During February there were four rain periods with precipitation varying from 0.07 to 0.78 inch. After each rain the number of viable pycnospores obtained from each trap varied from a few thousand to 92,000,000. Similar results have been obtained for the months of March and April.

The cotton for the traps was transported to the laboratory in sterile Petri dishes and the number of viable spores determined by the poured plate method, using 3 per cent. dextrose agar, plus 10.

One of the noteworthy facts is that ascospores do not appear to be washed down the tree during the winter rains, although they are present in abundance in the pustules. That all the colonies appearing in the poured plates made from the cotton traps came from pycnospores was demonstrated in two different ways: first, by their time of appearance; second, by the absence of ascospores in the centrifuged sediment as determined by microscopic examination.

The effectiveness of the cotton traps in retaining the spores washed down has also been determined by cultures. In many cases only about two per cent. of the spores passing into the cotton was retained. This being true, the figures given above are but a meager expression of the enormous numbers of pycnospores produced. Considering the fact that we have also demonstrated that pycnospores can be subjected to freezing temperatures for considerable periods without losing their vitality, they must play a very important part in the dissemination of the blight fungus.

The forcible expulsion of the ascospores of the blight fungus has been reported by Rankin[2] and Anderson[3] and the influence of moisture upon this phenomenon has been demonstrated by both writers. They have not, however, taken temperature conditions into account. The expulsion of ascospores depends not only upon the presence of sufficient moisture, but also upon the temperature to which the lesion has been subjected. The influence of temperature upon the expulsion of ascospores has been determined during the past winter by means of laboratory and field tests. Laboratory tests have shown that bark bearing perithecial pustules, if subjected to low temperatures (42 to 46° F.) for a period would not begin the expulsion of ascospores until exposed to favorable temperatures for three or four days, even though supplied with an abundance of moisture. The minimum temperatures at which spore expulsion takes place vary from 52 to 60° F.

On November 26, 1912, a large number of ascospore traps (49) were placed upon lesions of the blight fungus in a badly diseased coppice growth at West Chester, Pa. These have been under continuous observation since that time and accurate rainfall and temperature records kept at that station. There was practically no expulsion of ascospores until March 21, although there had been many rain periods (21) with precipitation varying from 0.01 to 1.64 inch. The above records show that ascospores were not washed down from the lesions during any of the winter rains and that there was practically no expulsion of ascospores during the period from November 26 to March 21.

F. D. HEALD
M. W. GARDNER

PENNSYLVANIA CHESTNUT TREE
BLIGHT COMMISSION LABORATORY,
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A STRIKING CORRELATION IN THE PEACH

THE importance of correlations in the characters of plants from the standpoint of either research or practise is so well known as to

[2] Rankin, W. H., Report Penna. Chestnut Blight Conference, p. 46, 1912. *Phytopathology*, 3: 73, 1913.

[3] Anderson, P. J., *Phytopathology*, 3: 68, 1913.

need no setting forth. The search for them is one of the imperatives with those who experiment or those who cultivate plants and once begun becomes fascinating—indeed irresistible. The seeker is so seldom rewarded—correlations are not common phenomena—that haste may be pardoned in publishing a discovery.

This spring a most striking correlation in the peach, which seems not to have been noted before, came to the attention of the writer. In the work of describing the flowers of a considerable number of varieties of peaches, Mr. Charles Tubergen, of the horticultural department of this station, found that there was a somewhat remarkable difference in the color of the inside of the calyx cups of different varieties. In some of the blossoms the color of the inner surface of the cup was green, usually a light green but varying somewhat in different varieties. In other varieties the cellular tissue of the inner part of the cup was a deep orange in color—not the surface alone but the tissue to the depth of several layers of cells was orange. In no other part of the calyx, the blossom, or the plant, in the spring of the year, does there appear to be a similar color.

Upon investigation it was found that the flowers having the green cup were those of the white-fleshed varieties while the blossoms with the orange cup were those of the yellow-fleshed ones. We were able to observe the character in two trees each of 307 varieties of peaches and of 47 varieties of nectarines. Of the peaches 145 varieties were white in flesh and green inside the calyx-cup; 162 were yellow in flesh and orange inside the calyx-cup. In the nectarines white and green were correlated in 36 and yellow and orange in 11 varieties.

In neither peach nor nectarine are there intermediates in color of calyx-cup or in flesh of fruit. The parentage of a sufficient number of the varieties examined is known to make it certain that green calyx-cup with white flesh and orange calyx-cup with yellow flesh are each inherited as one in cross-breds.

What is the explanation of this hidden connection between colors in two organs of the peach which are not only quite distinct but which appear on the plant in periods as widely separated as blooming-time and fruiting-time? The correlations are so constant and their hereditary behavior is such as to suggest that each is a single color character diffused through the flesh of the peach fruit and the inner tissue of the calyx cup. Surely the two organs in which the correlation appears are morphologic units but the capacity to produce the same color, differing probably only in degree in the parts in which it is found, and at widely different times, must be conceived to be a physiologic unit. If so, why localized in these two organs and not generalized throughout similar tissue in other parts of the plant as correlated colors generally are?

This correlation has some practical value in peach-breeding, since it will often enable the breeder to tell a year or two sooner than he otherwise could what color of flesh his peach will have since the first blossoms seldom set fruit; it is of material value in classifying peaches—adding another very constant taxonomic character; through its uses for the breeder and the systematist it becomes ultimately of considerable value to peach-growers. Lastly, it seems to the writer to have value in throwing light on current conceptions of morphologic and physiologic units in plants and also presents a problem to be explained as to why there is a localization of a particular color in two quite distinct organs.

U. P. Hedrick

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MICHIGAN ACADEMY OF SCIENCE

The Michigan Academy of Science held its nineteenth annual meeting at Ann Arbor on April 2, 3 and 4.

The following is the program as given at the meeting. The numbers marked with a star will be published in the Fifteenth Annual Report of the Michigan Academy of Science.

*"The Porcupine Gold Deposits of Ontario," Mr. R. E. Hore.

SECTION OF SANITARY AND MEDICAL SCIENCE

Thursday, April 3

*"Ferments," Dr. J. G. Cumming.

*"A Bacterial Disease of the Larva of the June Bug (*Lachnosterna* sp.)," Miss Zae Northrup.

"Duration of Tr. Gambiense Infection in Rats and Guinea-pigs," J. F. Morgan.

*"The Environment of Soil Bacteria," Dr. F. H. Hesselink van Suchtelen.

*"The Influence of *Bacterium Lactis Acidi* on the Changes caused in Milk by some of the Common Milk Microorganisms," C. W. Brown.

"The Use of Chlorinated Lime for the Disinfection of Drinking Water," Dr. M. L. Holm and E. R. Chambers.

*"Ozone as a Means of Water Purification," R. W. Pryer.

"Toxic Bases in the Urine of Parathyroidectomized Dogs," W. F. Koch.

"Serum Tests in the Diagnosis of Infectious Abortion of Cattle," Dr. E. T. Hallman.

"The Increase of Hog Cholera Virus by Intraperitoneal Injections of Salt Solution," W. S. Robbins.

"Studies in Avian Tuberculosis," L. R. Himmelberger.

"The Sensitizing Group in the Protein Molecule," Dr. V. C. Vaughan.

"Immunization against Tr. Brucei with Cultures," Dr. F. G. Novy.

"Determination of Minimum Lethal Dose of Tr. Brucei," C. A. Behrens.

"Cultivation of Spirilla," P. H. de Kruif.

*"Secret Remedies, Nostrums and Fakes," Dr. W. S. Hubbard.

SECTION OF ECONOMICS

Thursday and Friday, April 3 and 4

"The London Dock Strike of 1912," Carl E. Parry, of the University of Michigan. Discussion opened by W. H. Hamilton, of the University of Michigan.

*"Farm Organization as a Factor in Rural Economics," Wilbur O. Hedrick, of Michigan Agricultural College. Discussion opened by Edward D. Jones, of the University of Michigan.

"The Sphere of Pecuniary Valuation," C. H. Cooley, of the University of Michigan. Discussion opened by Frank T. Carlton, of Albion College.

*"Psychological Antithesis of Socialism," H. A. Miller, of Olivet College.

GENERAL PROGRAM

Presidential address, by Professor E. C. Case: "The Geological History of Michigan."

Reports on the work of the Michigan Geological and Biological Survey, by R. C. Allen, director, and A. G. Ruthven, chief naturalist.

Papers on Eugenics

"Eugenics," Professor Victor C. Vaughan, department of medicine, University of Michigan.

"The Biological Aspect of Eugenics," Professor A. Franklin Shull, department of zoology, University of Michigan.

Public address, "Travels in Mexico," by Professor Charles J. Chamberlain, department of botany, University of Chicago.

The Research Club of the University of Michigan gave a smoker to the members of the academy in the rooms of the University Club, Memorial Building.

SECTION OF GEOLOGY AND GEOGRAPHY

Thursday, April 3

*"Origin of Continental Forms, IV.," Dr. Howard B. Baker.

*"Studies in Structure and Stratigraphy in the Saginaw Valley in Relation to Occurrences of Oil and Gas," Mr. R. A. Smith.

"Climatic Variation in Permian Times as recorded in Red Beds of Texas," Professor E. C. Case.

"The Discovery of Illinoian Till in the Detroit River Region," Professor W. H. Sherzer.

"The History of Lake Erie in Post-Glacial Time," Mr. Frank B. Taylor.

"Further Studies on the Variation of the Angle of the Optic Axes, with Temperature," Professor E. H. Kraus.

"Vanadiferous Pyroxenes from Libby, Montana," Professor W. F. Hunt.

"Some Pro-Glacial Lake Shore Lines of the Bellevue Quadrangle, Ohio," Professor Frank Carney.

"Some Problems in Stratigraphy and Correlation of the Pre-Cambrian Rocks of Michigan," Mr. R. C. Allen.

(*a*) "Results of Leveling along the Algonquin Beach in the Northern Peninsula in 1912," (*b*) "Order of Development of Glacial Lakes in the Great Lakes Region," (*c*) "Centers of Dispersion and Probable Extent of the Kansan and Pre-Kansan Drifts," Mr. Frank Leverett.

"The Chemical Composition of Bornite," Professor E. H. Kraus and Mr. J. P. Goldsberry.

"The Teaching of Economics in the High School." Discussion opened by J. E. Mitchell, of Alma College, and F. M. Taylor, of the University of Michigan.

*"The Taxation of Local Public Utilities in Michigan," E. H. Ryder, of Michigan Agricultural College.

"Public Utility Accounting in Michigan," David Friday, of the University of Michigan. Discussion opened by H. C. Adams, of the University of Michigan.

SECTION OF ZOOLOGY

Thursday, April 3

"Factors Governing Local Distribution of the Thysanoptera," A. F. Shull.

"Results of the Mershon Expedition to the Charity Islands, Lake Huron Coleoptera," A. W. Andrews.

"Types of Learning in Animals," J. F. Shepard.

"The Lepidoptera of the Douglas Lake Region, Cheboygan County, Michigan," Paul S. Welch.

"Check-list of Michigan Lepidoptera. II. Sphingidæ (Hawk Moths)," W. W. Newcomb.

*"On the Breeding Habits of the Log Perch," Jacob Reighard.

"A List of the Fish of Douglas Lake, Cheboygan County, Mich., with Notes on their Ecological Relations," Jacob Reighard.

"May the Remains of Adult Lepidoptera be Identified in the Stomach Contents of Birds?" F. C. Gates.

"The Mitochondria," R. W. Hegner.

"The Unione Fauna of the Great Lakes," Bryant Walker.

"Notes on the Genus *Edaphosaurus* Cope," E. C. Case.

"Methods of Preparing Teleost Embryos for Class Use" (demonstrations), B. G. Smith.

*"An Adult *Diemyctylus* with Bifurcated Tail," B. G. Smith.

"Notes on the Mollusks of Kalamazoo County, Mich.," Harold Cummins.

*"Sarcoptid Mites in the Cat," Harold Cummins.

*"The Origin of Continental Forms, III.," Howard Baker.

"An Ecological Study of the Birds of Manchester, Mich.," F. Gaige.

"Notes on Crustacea Recently Acquired by the Museum of Natural History of the University of Michigan," A. S. Pearse.

"Distribution of Multiple Embryos on the Blastoderm," O. C. Glaser.

*"Nesting of Our Wild Birds," Jefferson Butler.

*"The Factors that Determine the Distribution of *Boleosoma nigrum* in Douglas Lake, Cheboygan County, Mich.," H. V. Heimburger.

"Structure of the Olfactory Organs," E. W. Roberts.

"A Method of Producing Cell-like Structures by Artificial Means," E. W. Roberts.

"Some Notes on Rhizopods from Michigan," E. W. Roberts.

"An Interesting Form of Protozoa," E. W. Roberts.

*"Oxygen and Carbonic Acid Contents of Douglas Lake, Cheboygan County, Mich.," D. A. Tucker.

"Some Observations on *Asplanchna amphora*," D. A. Tucker.

"Some Effects of Sunlight on the Starfish," H. M. MacCurdy.

"Some Abnormalities Observed in Proteocephalid Cestodes," G. LaRue.

"Note on a Cestode Found in a Garter Snake," G. LaRue.

*"Some Observations on Intestinal Villi," O. M. Cope.

"Some Physiological Changes in the Lamprey Egg after Fertilization," P. Okkelberg.

"A Collection of Fish from Houghton County, Mich.," T. L. Hankinson.

"The Lagoons and Ponds of Douglas Lake, Cheboygan County, Mich.," H. B. Baker.

"The Shiras Expeditions to Whitefish Point, Mich.": (1) "Birds," N. A. Wood. (2) "Mammals," N. A. Wood. (3) "Amphibians and Reptiles," Crystal Thompson and Helen Thompson.

"Notes on the Ornithology of Clay and Palo Alto Counties, Iowa," A. D. Tinker.

"A Check-list of Michigan Mammals," N. A. Wood.

"The Variations in the Number of Vertebræ and Ventral Scutes in the Genus *Cegina*," Crystal Thompson.

"An Artificially Produced Increase in the Proportion of Male Producers in *Hydatina senta*," A. F. Shull.

SECTION OF BOTANY

Thursday, April 3

"Biometric Studies in Oaks" (with lantern), Carl Oberlin.

"Biometric Studies in Oaks" (with lantern), J. H. Ehlers.

"The Origin of *Capsella arachnoidea*" (with lantern), Henri Hus.

*"The Antitoxic Action of Chloral Hydrate upon Copper Sulphate for Pea Seedlings," R. P. Hibbard.

*"Improved Methods for the Quantitative Determination of Dilute Solutions of Electrolytes," R. P. Hibbard.

"Effect of Illumination on the Twining of Plants," F. C. Newcombe.

"Conditions for the Diageotropism of *Asparagus plumosus*," Margaretta Packard.

"A Heteroprophic Mycorhiza" (with lantern), Walter B. McDougall.

*"Some Notes on the Black Knot of Plums," J. A. McClintock.

"Some Further Observations on *Sclerotinia*," J. B. Pollock.

"A Sand-binding Fungus," J. B. Pollock.

"The Relic Dunes of Little Point Sable" (with lantern), W. E. Praeger.

*"The Pine Hills at Lowell, Mich." (with lantern), Bert E. Quick.

"Plants observed on Mackinac Island in 1912," C. K. Dodge.

*"The Flora of Parkdale Farm, Rochester, Mich.," O. A. Farwell.

"The Early Extent of Prairies in Southern Michigan," H. A. Gleason.

*"Notes on a Few Plants from the Vicinity of Ann Arbor," H. A. Gleason.

*"Car-window Notes on the Vegetation of the Upper Peninsula," R. M. Harper. Read by H. A. Gleason.

*"Permanent Vegetation Quadrats at Douglas Lake," Ada K. Dietz.

*"Rôle of Vegetation of a Mill Pond" (with lantern), F. A. Loew.

*"Key to the Species and Varieties of *Solidago*, in Michigan," C. H. Otis.

*"An Easy Formula for Obtaining Alcohols of any Strength," Richard de Zeeuw.

"Lipolytic Action in a Rust," G. H. Coons.

"Soft Rot of the Hyacinth," G. H. Coons.

RICHARD DE ZEEUW,
Secretary

EAST LANSING, MICH.

SOCIETIES AND ACADEMIES

THE BOTANICAL SOCIETY OF WASHINGTON

THE eighty-ninth regular meeting of the Botanical Society of Washington was held in Assembly Hall of the Cosmos Club, at 8 P.M., Tuesday, May 6, 1913, with twenty-four members and two guests present.

The following papers were presented:

The Effect of the Recent Freeze in California (with lantern): Dr. DAVID GRIFFITHS.

Dr. Griffiths discussed the effect of the January freeze on vegetation of the southwest, with special reference to California. The main regions where tropical and subtropical things are being grown were visited. He showed 40 slides made from negatives taken in February and March, showing injuries to citrus fruits, avocados, cherimoyas, mangoes, carobs, acacias, olives, eucalyptus, etc.

While the temperatures were unusually low, there are indications that they have been lower in the remote past. That such cold spells of weather are very infrequent is proved by the fact that such natives as *Rhus laurina*, eriogonums and other natives in California, and the giant *Cereus, cholla, Celtis, Olneya*, etc., in southern Arizona, are severely injured. Many introduced trees which have attained a diameter of three feet have been killed outright.

Injuries were very severe throughout all of the citrus regions, but even where the temperatures went to 10–17° F. in general throughout a region, an occasional orchard situated upon an abrupt elevation above the general plain escaped with even unfrozen fruit. Owing to differences in elevation, air-drainage and exposures, conditions are exceedingly varied and present some of the most important problems in connection with the relation of climatic conditions to crop development. At no time in the present generation has there been such an opportunity to determine the adaptability of the scores of introduced plants of the Pacific Coast region. Through some of the various agencies operating in agricultural lines a careful survey should be made the present season to systematize and place on record the results of a condition which, although of infrequent occurrence, is nevertheless of the utmost scientific and economic import.

The Method of Types Applied to the Nickernut: Mr. H. C. SKEELS.

Mr. Skeels called attention to the last sentence of division (*e*) under Canon 15 of the American Code of Botanical Nomenclature, which reads as follows: "The genera of Linnæus's Species Plantarum (1753) are to be typified through the citations given in his Genera Plantarum (1754)." Under this clause the following genera were mentioned:

Genus	Type Species	Now Referred to
Alpinia	*A. racemosa*	*Renealmia*
Cerbera	*C. ahouaj*	*Thevetia*
Cratægus	*C. aria*	*Sorbus*
Cucurbita	*C. lagenaria*	*Lagenaria*
Glycine	*G. apios*	*Apios*
Hibiscus	*H. malvaviscus*	*Malvaviscus*
Jatropha	*J. manihot*	*Manihot*
Medicago	*M. radiata*	*Trigonella*

Applying the method of types to the nickernut, Mr. Skeels called attention to Mr. Trimen's identifications of the Flora Zeylanica specimens which are published in Vol. 24 of *The Journal of the Linnean Society, Botany.* On the basis of these identifications, Mr. Skeels concluded as follows:

"In conclusion, going back to our three original species, the 'Catti kitsjil' of the East Indies, the *Cæsalpinia nuga* (L.) Aiton of the floras, under the method of types of the American Code, becomes *Cæsalpinia crista* L., the type being Fl. Zeyl. 157. The common gray-seeded nickernut, generally known as *Cæsalpinia* or *Guilandina bonducella,* becomes *Guilandina bonduc* L., the type being Fl. Zeyl. 156. And the yellow-seeded, large-leafletted nickernut, generally known as *Guillandina bonduc,* becomes *Guilandina major* (DC.) Small, being based through De Candolle, on *Guilandina bonduc* L. Species Plantarum, ed. 2."

What would be the Effect of the Arctic Night on Tropical or Subtropical Vegetation? Dr. F. H. Knowlton.

Dr. Knowlton called attention to the ancient floras of the North Polar region. Many of the fossil plants found there are of a tropical or subtropical character. No satisfactory explanation of the relation of such plants to the conditions of light and darkness supposed to have prevailed has been found. Dr. Knowlton asked for suggestions from the members of the society and a brief discussion followed.

C. L. Shear,
Corresponding Secretary

Philosophical Society, University of Virginia
Mathematical and Scientific Section

The eighth regular meeting of the session of 1912–13 of the Mathematical and Scientific Section was held May 19.

Professor R. M. Bird read a paper by himself and Mr. W. S. Calcott on "Some Studies of Chemical Reactions, which may be Connected with the Constant Association of Vanadium Sulphide with Sulphur-bearing Petrols and Asphalts."

Mr. Justus M. Cline presented a paper by himself and Professor Thos. L. Watson on "The Drainage Changes in the Shenandoah Valley Region in Virginia."

Professor Thos. L. Watson read a paper entitled "A Meteoric Iron from Paulding County, Georgia."

Professor Chas. N. Wunder read a paper on "A Photometric Survey of the Stars of the Huygenian Region of the Great Nebula of Orion."

Professor W. H. Echols read by title a paper "On the Expansion of a Function in Terms of Rational Functions." This paper will be read at the regular meeting in October.

Wm. A. Kepner,
Secretary

University of Virginia

Science Club of the University of Wisconsin

At the 125th regular meeting of the club, held April 17, 1913, Dr. A. S. Loevenhart, of the department of pharmacology of the University of Wisconsin, presented a paper on "The Relation of Oxidative Processes in the Central Nervous System to Stimulation and Depression."

The observed effects of asphyxia are, in the order of occurrence, stimulation, depression, paralysis, death. By using carbon monoxide, hydrocyanic acid and other drugs that inhibit the oxygen-carrying power of the blood without interfering with the elimination of carbon dioxide, and by reducing the time of the experiment so much that no accumulation of acid products is possible, it is found that decrease of oxidation *per se* is responsible for the initial stimulation observed in asphyxia.

Increased oxidation, secured by the use of certain derivatives of iodbenzoic acid, results in suspension of respiration and other evidences of depression of the central nervous system.

Anesthesia is a case of secondary depression resulting from decreased oxidation as opposed to depression from increased oxidation, which is probably the condition occurring in sleep. A "safe" anesthetic is therefore an impossibility, since the function of an anesthetic is to hold bodily oxidation down in the region of depression between stimulation and death.

The lecture was illustrated with a number of diagrams from automatic recording apparatus, and some pieces of apparatus that had been specially designed or improved in connection with the investigation.

Eric R. Miller,
Secretary